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CONCEPTUAL FRAMEWORKS IN PSYCHOLOGY: A MODIFIED KUHNIAN
ANALYSIS OF THE EMERGENCE OF DOUBLE BIND FAMILY THERAPY

A Dissertation Presented

By

DENISE J. GELINAS

Submitted to the Graduate School of the
University of Massachusetts in partial fulfillment
of the requirements for the degree of

DOCTOR OF PHILOSOPHY

May 1978

Department of Psychology



Denise J. Gelinas 1978

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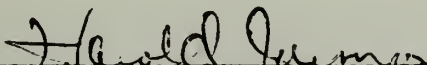
By

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Peter Park, Member



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Normal F. Watt, Chairperson
Department of Psychology

Dedicated to
Dr. Harold Raush

To thank him for his, sometimes
passionate, belief in the
necessity of plurality in science,
and for his integrity...

The following currently popular vignette suggests itself as a metaphor for following some of the ideas contained herein.

A scientist was studying jumping behavior in the frog, so he placed a frog on a measured grid.

"Jump frog. Jump". The frog jumped, and in his notebook, the scientist wrote: Frog with four legs jumps six feet. So the scientist cut off the frog's front leg.

"Jump frog. Jump". And the frog jumped. In his notebook, the scientist wrote: Frog with three legs jumps four feet. So the scientist cut off its other front leg.

"Jump frog. Jump". And the frog jumped. In his notebook, the scientist wrote: Frog with two legs jumps two feet. So by now the pattern being clear, the scientist cut off its two remaining legs.

"Jump frog. Jump". But the frog didn't jump.

"Jump frog. Jump". But the frog still didn't jump, so the scientist wrote in his notebook. Frog with no legs - is deaf.

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In several ways this dissertation is a summary statement of my graduate education and, as such, it shows the influence of several people to whom I owe thanks.

Harold Raush's teaching, research, and example widened the boundaries of what is recognized by many as legitimate and substantial scholarship; because of his continuing influence the present sort of work is possible, conceptually and practically.

Howard Gadlin agreed to chair this dissertation when it had no set form or conclusions. He helped me to conceptualize and present the material in a manner that met the accepted standards of scholarship without insisting upon the usual format. The dissertation reflects a good deal of his thinking and practice in which psychology is considered a responsible science. Also, his flexibility and support during a particularly grim time, and his understanding of what I was attempting to do, made this dissertation possible.

Hal Jarmon's ability to relate the present meta-scientific analysis to processes and phenomena encountered in clinical practice validated and enriched this dissertation. Also, his insistence upon the use of appropriate methods and approaches in the research of clinical processes have permeated the dissertation.

Al Winder has provided a wide-ranging sophistication of theory and clinical practice that has informed my analysis, and at times, supported it in unexpected ways. His use of the present analysis for a wholly different area in which he is involved highlighted strengths and weak-

nesses in a manner that was supportive while being rigorous.

Peter Park has, in practically every meeting, furthered the conceptual base of my analysis, extending it to new areas, and expanding certain points to see them more clearly or to see what new interpretations they could provide.

Thanks are also owed to friends for their frequent, and appreciated, support. Here I would like to thank especially Allison Cook, Marcia Howard and Joel Feinman, Harold Seewald, and Adin Dela Cour. Sue Gottlieb and I spent several months meeting for suppers and library sessions; Elaine Faunce shared with me some of her experiences and materials from her work at MRI. These friends gave me support and affection, and understood without pique, my inability to spend time or energy with them.

My special thanks go to my partner Mark Karpel for his wholehearted support during the many months of this dissertation's making. His appreciation of the difficulty of working while writing a dissertation, and his practical as well as emotional support were invaluable. I thank him for his intellectual and emotional companionship.

Finally, my thanks go to Mrs. Doris Maynard, who typed and often corrected, a 450 page manuscript for me in thirteen days - and all the while maintained a bouyant, if progressively fatigued, demeanor.

ABSTRACT

Conceptual Frameworks in Psychology: Modified Kuhnian Analysis for the Emergence of Double Bind Family Therapy

May, 1978

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The presence of a felt crisis in psychology is reviewed in its various manifestations: concern about lack of long-term progress, questions of sub-disciplinary identity and the presence of long-standing "sterile" controversy. Previous attempts to use Thomas Kuhn's schema for scientific change to interpret this situation have met with little success because no provisions had been made to modify the schema for use with a social science. In the present investigation, these provisions were made and the modified Kuhnian analysis was used to interpret events leading up to, during, and immediately after the emergence of family therapy. This Kuhnian analysis made it possible to see that family therapy had arisen in reaction to classical psychoanalysis' inability to deal with certain clinical phenomena, including neurotic complementarities among married couples, the precipitation of psychoses in borderline personalities, and the homeostatic dynamics in the families of schizophrenic patients. Controversies between classical analysts and Sullivanian analysts in the 1950's are interpreted in Kuhnian disciplinary matrix

terms rather than methodological terms.

The development of the "double bind hypothesis" by Bateson, Jackson, Haley, and Weakland is interpreted as meeting the necessary criteria for a Kuhnian revolutionary paradigm. Then the ten-year formal history of Bateson's research group is reviewed and interpreted in disciplinary matrix terms. The elaboration of the communicational and homeostatic aspects of the paradigm and the major lines of activity are documented, as well as the 1959 split. Interpretation in disciplinary matrix terms also helps to explain how the group was able to continue productive work for several years after splitting in 1959.

Certain elaborations of the Kuhnian analysis are then offered and found to be helpful in understanding the inception and development of the double bind paradigm and disciplinary matrix. These modifications emphasized the group structure of these activities and the importance of a shared disciplinary matrix for communication and the resolution of controversy. Several ostensibly methodological controversies involving classical and Sullivanian psychoanalysts and double bind adherents were interpreted as controversies between disciplinary matrices and therefore not resolvable if argued on the methodological level. In a Kuhnian analysis, methodology is not independent from a disciplinary matrix. Also, an emphasis on the group structure of scientific activity combined with the paradigm concept allowed an interpretation of the relationship among the classical psychoanalysts, the Sullivanian analysts and family therapists during the early 1950's.

Two new concepts were proposed. The first was termed the "border of applicability"; it provides a criterion, created by emerging anomalies by which to recognize the reasonable limits of usefulness for a particular paradigm. The second proposed concept was the "meta-disciplinary matrix" the meta-disciplinary matrix is a philosophical/conceptual constellation which includes a philosophical orientation (e.g., logical positivism, or dialectics) level of phenomena of focus (e.g., behavioral, or phenomenological), value systems (e.g., predictive, or interpretive understanding), and models of explanation (e.g., mechanistic, organismic, or formalistic). It was proposed as an explanation of why several family therapy disciplinary matrices "clustered" together. As an example, the double bind matrix was shown to share the same mechanistic, behavioral and predictive preferences as Minuchin's structural family therapy group.

The meta-disciplinary matrix concept was also used to help explain along what dimension the first "cluster" differed from the second "cluster" identified - one formed by the disciplinary matrices of Ackerman, Boszormenyi-Nagy, and Laing. This second cluster is characterized by a preference for phenomenological plus behavioral thinking, and a preference for interpretive understanding. The meta-disciplinary matrix is seen as preceding and subsuming paradigms and disciplinary matrices. It was proposed that the present felt crisis in psychology was amenable to a modified Kuhnian analysis if one focused on the differences and relationships among, paradigm, method, disciplinary matrix, and meta-disciplinary matrix.

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I N T R O D U C T I O N

During the several chapters following, I hope to demonstrate there is a felt crisis in the discipline of psychology, and to propose a modified Kuhnian analysis to interpret this felt crisis. As will be seen the presence of a felt crisis is being expressed in a number of ways. Some are concerned about a lack of long-term progress, while others point to an episodic quality in the development of the discipline whereby research results and conceptualizations rise and fall but very seldom accumulate with a consensually validated, lasting body of knowledge. Similarly, others have drawn attention to crises of identity, either in the discipline as a whole, or in sub-disciplines. Finally, other authors are challenging a number of philosophical and methodological characteristics of the discipline, identifying areas of repeated problems and suggesting alternatives.

During the development of the discipline and these issues, controversy and debate have been rife, and not always productive; further, some of this type of accompanying controversy has seemed somewhat beside the point though very characteristic of the period. Several authors (Burgess, 1972; Watson, 1974; Stierlin, 1977) have used the conceptual schema originated and developed by Thomas Kuhn in The Structure of Scientific Revolutions (1970b) to understand change and progress in science. However, there are problems in using Kuhn's work for psychology - these will be discussed at length in the ensuing chapters. It is important to note here, however, that Kuhn's ideas have often been used uncritically

and without taking into account his subsequent important revisions; nor has his framework been critically reviewed and adapted for use in psychology. Consequently, his schema has occasionally been used as a weapon in just the sort of sterile controversy already mentioned.

Burgess (1972, p. 193) documents just one such sequence; referring to the publication and revision of Scientific Revolutions, Burgess comments:

In one sense, at least, this publication has united all kinds of psychologists. Anti-behaviourists now believe they have the final weapon with which they will eventually demolish that monolithic anti-Humanitarian monster--Behaviourism (Jenkins, 1968; Koch, 1964). Behaviourists now believe they have fired the last salvo in their battle against "extinct" Psychoanalysts (e.g. Krasner, 1971), and so it progresses. One wonders, however, whether these theorists are not attacking straw men which they have erected themselves (a favourite armchair pasttime of psychologists who have managed to identify with one or other "father-figure-school"). Of more importance though is whether psychologists generally have read Kuhn correctly or are showing a familiar bandwagon effect (as Koch, (1964) has shown for the early learning theorists in their slavish imitation of the logical positivists). It seems that Psychologists are especially susceptible to this sort of thing, i.e. picking up a thread midway, being neither prepared to return to the original ball of string or to see where eventually their thread leads or to what it is attached.

Despite these problems regarding Kuhn's schema for psychology, his work may potentially provide insight and a structure within which to interpret events. Of particular interest for the present felt crisis in psychology is Kuhn's emphasis on crisis, disagreement, and development rather than accretion, in science.

In the following chapters, I will use a modified Kuhnian analysis for one substantive area in which there occurred a felt crisis, to see if the "application" of the Kuhnian schema allows us to interpret or

understand a crisis situation in psychology, and especially if Kuhn allows us to interpret events in a different manner than previously, or even to find sense in heretofore senseless or random-seeing processes.

The "substantive area" of focus will be the emergence of family therapy, specifically the emergence of the double bind hypothesis and double bind (DB) family therapy. I have used the term "substantive area" rather than "discipline" because, while family therapy is a subdiscipline in psychology, it is also a sub-discipline in psychiatry, social work, nursing, and education with some adherents and practitioners are also occasionally found in anthropology, sociology, and psychosomatic medicine. The criteria for defining the area and the individual's inclusion within it, is neither based upon credentials of training nor discipline, but rather on adherence to the idea that within the family lie the processes to which we can attribute the etiology, maintenance, and potential for amelioration of psychological (and some think, psychosomatic) disorder. This set of ideas is a marked departure from intra-psychic formulations based upon and within the individual. It should be acknowledged that family therapists have little trouble recognizing each other, though agreeing with each other is more of a problem. This larger complex of family therapists occurs across disciplines, in terms of their own perceptions of the family field and who "belongs" in it, and also because almost any substantive area in psychology cuts across formal disciplinary lines and can be found in neighboring disciplines. The substantive area rather than the disciplinary name is the more

sensible boundary for the present analysis.

The emergence of DB family therapy was chosen for a number of reasons. With its recent differentiation from individual psychotherapy during the past quarter-century, family therapy has a fairly accessible history; it is relatively well-documented, with many, though not all, of its originators currently active in the field.

Family therapy in its entirety, however, was not chosen as the focus of analysis because it is constituted by a number of groups, theories and sets of practice, so that, as an area, it is also subject to some degree of disagreement and controversy. It seems not surprising that within only a quarter-century, there has already emerged debate and groups in disagreement. Also, it is apparent that the term "family therapy" is applicable to a wide range of practices, and that it is used in a broad, rather vaguely defined manner (Freeman, 1964, p. 35). For these reasons, one of the family therapy frameworks, rather than the entire discordant field, appeared preferable for a detailed analysis. Double-bind family therapy was one of the frameworks that developed during the 1950's and 1960's, and an argument will be made that it constituted the first systems approach and the first family framework to embody the full characteristics of family therapy rather than merely transitional elements. Double-bind family therapy is commonly, and justifiably, regarded as constituting a unit in itself, having internal consistency and generally recognized characteristics. Hence, it was chosen for analysis.

It could be pointed out that, unlike the physical sciences, family therapy is constituted not only by research and theory, but also by

clinical practice. There are questions as to whether it is legitimate to use a schema designed from, and for, the physical sciences, as Kuhn's is, for a social science area that also includes a clinical practice component. It would be possible to make an argument that clinical practice itself can constitute a legitimate form of research, and some (Raush, 1974; Sullivan, 1953) have done so. As this particular point is not the primary focus of this dissertation, however, I will remain with the original issue about the use of Kuhn for areas which include clinical practice; specifically, will it "work" when I attempt a Kuhnian analysis for family therapy, or will such an application strain the Kuhnian schema such that it can be inferred the analysis is not appropriate. This appears to be a question best answered by attempting the application.

Procedurally, a modified Kuhnian analysis will be used to interpret the events, processes, and controversies surrounding the emergence and development of DB family therapy. Hopefully, this analysis will shed light, or meaning, on events which had previously appeared merely random. If successful, this will indirectly suggest where the Kuhnian framework is appropriate for psychology and similar disciplines, and where the schema needs revision in view of difficulties encountered in the process of such an application. It is very probable that, in applying the Kuhnian schema to family therapy, information about his schema will also emerge. It is expected that use of the framework will highlight its strengths and weaknesses as well as providing information about a sub-discipline partially within psychology; that is, the process of examining a framework

in psychology will in turn reveal information about the conceptual tools originally used for the analysis, and these secondary findings will also be discussed.

This method, in which the process of examining the primary subject area, in turn reveals information about the conceptual tools, is a variant of what Radnitzky (1973) refers to as "tacking." (Radnitzky discusses dialectical theory as proceeding in "turns" first emphasizing the empirical human sciences and then emphasizing critical social theory, alternating or "tacking" back and forth, so that they shed light on each other).

Some of this tacking back and forth has already taken place; for instance, the attempt to apply Kuhn's work to the informational sciences resulted in Masterman's (1970) revision of Kuhn's work on a multiple-paradigm issue. (See Chapter II). This differs from the situation in which Masterman explicitly addresses the paradigm concept to critique it. Though "tacking" entails a certain amount of methodological complexity, the information reflected back upon the conceptual tools should not be left by the wayside but should instead be considered a secondary or subsidiary focus.

Schematically, in Chapter I the felt crisis in psychology will be explicated. In the second chapter, Kuhn's framework regarding scientific developments will be reviewed, along with critiques and revisions from the literature, and necessary modifications to allow legitimate application of the scheme to the social sciences. In the third and fourth chapters, the focus will shift to examine the developments in psycho-

analysis leading up to the introduction of the DB framework.

Chapters V and VI will explicate the inception of DB family therapy as a Kuhnian revolutionary paradigm, then trace the development of the DB developers as a research and clinical group. In the seventh chapter, the analysis carried out thus far will be examined as to its strengths and weaknesses, and used to interpret several controversies in which the DB group members had engaged.

Finally, in the eighth chapter, proposals for further modifications in the Kuhnian schema will be forwarded, as well as suggestions for further analyses regarding psychology's felt crisis.

C H A P T E R I

FELT CRISIS IN PSYCHOLOGY AS A DISCIPLINE

It will become evident during the course of this chapter that there is currently a felt crisis in psychology (Becker, 1968; Farberow, 1973; Warren, 1971). Psychologists cannot agree on past achievements and continue to debate about fundamentals (Burgess, 1972, p. 198). Taking a reviewer's perspective, Buss (1975, p. 977) states that,

While reading current issues of psychological journals ... which promote discussion of general topics, trends, and controversies within the discipline (e.g., the American Psychologist), we cannot help but conclude that there are a significant number of professional psychologists concerned with where we are, how we got there, and where we are going from here.

Lest we think these considerations are rather complacent or of recent origin, Hudson (1972, p. 73) points out that the criticisms and concerns have become,

increasingly vociferous. Fifteen years ago, the vast majority of those working in psychological departments here and in the United States shared the belief that their discipline was robust, that their efforts embodied the onward march of Science. However, in the early 1960's a number of the informed and eminent, Sigmund Koch, for instance, were making sounds of misgiving, suggesting that for reasons of scientific insecurity, psychology was in retreat from its historically constituted subject-matter.

It would appear, further, that the perception of crisis spans the sub-disciplines of psychology, and is one of the few non-parochial elements of the discipline; it should be further noted, though, that within these sub-disciplines, it is primarily those who are investigating human actions (or cognition, relationships, etc.) rather than animal behavior

or neuropsychology, for example, that are most concerned about this crisis.

Naturally, different authors address themselves to different aspects of the crisis (or perceive different crises?) in psychology's development as a discipline; while this undoubtedly reflects psychology's history of factionalism, it also suggests the depth and extent of the crisis.

Lack of long-term progress and episodic patterns. Several perceive a crisis because of a lack of long-term progress in the discipline.

"Some thirty years ago, research in psychology became dedicated to the quest for nomothetic theory ... model building and hypothesis testing became the ruling ideal, and research problems were increasingly chosen to fit that mode. Taking stock today, I think most of us judge theoretical progress to have been disappointing (Cronbach, 1975, p. 116)."

Vitelis (1972, p. 601) is of the opinion that during the past 50-75 years, what psychologists have learned with any confidence about human behavior, is considerably more limited than might be anticipated from the number of publications in psychology.

Singer, in a lesson in brevity, states, "Some thirty years have passed, and we do not as yet have a developed, self-conscious discipline of a science of science (1971, p. 1010)." A partial solution to this state of affairs has been proposed by Elms (1975, p. 974) whereby, temporarily, "educative" articles examining the discipline's scientific and philosophical foundations would replace the usual empirical research reports in journals. Limitation of empirical research reports to coherent series

of studies, or to pre-planned strategic replications could free the space necessary for the non-empirical articles. He feels Moscovici's (1972) suggestion that data collection temporarily halt might be extreme.

The authors concerned about lack of long-term progress have voiced their disquietude and occasionally their recommendations, from essentially a sympathetic position. There have been others, sharing their concern, who have been more sharply critical and distanced: C. Rogers for example (1973, p. 379),

Psychology, for all its thousands of experiments, its multitudes of white rats, its vast enterprises involving laboratories, computers, electronic equipment, highly sophisticated statistical measures, and the like, is in my estimation slipping backward as a significant science. We have failed dismally to heed Robert Oppenheimer's warning, addressed to the APA in 1956, when he pointed out that the worst thing psychology might do would be "to model itself after a physics which is not there any more, which has been outdated [p. 134]."

Trenchant comment is also found within the experimental psychology tradition. Hudson (1972, p. 168) points out that as early as 1956, O.L. Zangwill believed that "Experimental psychology has produced many facts, a few generalizations, and even an occasional 'law.' But it has so far failed to produce anything resembling a coherent and generally accepted body of scientific theory." Similarly, Hilgard and Bower (1966, p. 424) in one of their textbooks on learning -- an unusual place for such a statement -- assert:

The argument has been made that more complex behaviors -- thinking and problem solving -- could be more easily understood once simple behaviors under especially simplified conditions were better understood .. After some thirty or forty years without

striking advances in our understanding of the capabilities of the human mind, this argument has begun to have a hollow ring.¹

There are a small number, as yet, who are calling attention to a specific pattern in this lack of long-term progress. Raush (personal communication, 1976) has pointed to a peculiar episodic quality to research and theory in psychology, specifically, to a pattern whereby theories or research lines are not necessarily invalidated and put aside for explicit reasons, but rather are "dropped" or are allowed to wither, so that a decade later, one wonders a bit "Whatever happened to ___?" The point is that there results no closure on this line of investigation; it is neither disconfirmed and then dropped, nor supported and consistently followed as a fruitful line of research. In a very clear example

¹At this juncture, it should be mentioned that not only psychology but all of the social sciences have been faulted for the lack of progress in the face of so much productivity:

Measured against the needs of the times, there is nothing remotely resembling a science of man [sic]; there are only mountains of disciplinary journals, and hordes of busy specialists; what is the effectiveness in relation to the momentous problems of survival and human dignity in our time? To ask the question is already to answer it; taken separately, most of the disciplinary activity in the social sciences represent trivial work. True, it is hard-working, certainly well-intentioned, at times deeply hopeful and anxious -- but still, somehow very much besides the point ... (Becker, 1968).

While I agree with Becker entirely, I will restrict my context to psychology, and my focus to a sub-discipline of psychology in the interests of clarity, manageability and commonsense.

of this sort of thing, Hilgard and Bower² in their 1966 revision, explicitly note the elimination of a theory from their text because of lack of interest, and not because the theory had been empirically discredited. Similarly, Bonneau (1975, p. 799) laments the "clusters of specialized research problems that arch gloriously through the higher intellectual atmosphere of scientific meetings and then like the Nehru shirt fade away to remain only in the memories of a few." Hudson's (1972, p. 55) Cult of the Fact returns to this episodic pattern a number of times, addressing several of its aspects:

Psychology proceeds more by fits and starts; a series of lunges into the surrounding darkness...a subject, or series of S's, in which one research fashion succeeds another, leaving little behind it as a residue of reusable knowledge (p. 55).. the impression is one of impermanence. There is change certainly; one vogue follows another. But the movement is less cumulative than cyclic; and more subject than in the other sciences to that 'Great Prime Mover of all intellectual activity, the Zeitgeist, without whom no man [sic] would think as he does, nor have his thoughts make sense (p. 156).'³

Crises of identity. Another group of authors has come to cite crisis, not so much because of lack of substantial progress, as because they perceive a crisis of identity, either in the discipline as a unit or within their particular sub-disciplines. Engineering psychology is

²Hilgard and Bower, 1966, pp. V, Vi and Vii.

³Hudson quoting from E.G. Boring, Sensation and perception in the history of experimental psychology; New York; Appleton Century Crofts, 1949, p. XI.

felt to be "in jeopardy" (Adams, 1972, p. 615); and while the problem in personality is "pressing," it is as urgent in cognitive (Cronbach, 1975, p. 120).⁴ Social psychologists appear to be questioning fundamental commitments to research approaches (Fried, et al., 1973, p. 155), and Elms (1975, p. 968) documents the "widespread self-doubts with goals, methods, and accomplishments" in personality research, developmental psychology, and clinical psychology, as well as social psychology. Among clinical psychologists, Albee (1970) has questioned the viability of the scientist-practitioner identity while Farberow (1973, p. 391) has offered a compromise in the practitioner-scientist model, and has advocated a significant change in APA structure because of its size and fragmentation.

The issue of the scientist-practitioner model and practice has not yet been resolved to some people's satisfaction, that is, to the satisfaction of those, usually, who have at least one foot in the practitioner's realm. Tyler (1973) and Hudson (1972) bring up particularly interesting points pertinent to the longevity of the dissatisfactions with this model.

It was recognized, of course, that there was such a thing as applied psychology, and applied psychologists of the clinical, counseling, industrial, school, and other varieties were trained in universities. These specialized kinds of

⁴Cronbach cites Newell (1972) on the fragmentation in information processing research alone, where the latter counted 59 different "colonies" of investigators, each collecting data on their own narrowly defined task.

activity, however, were not accorded the prestige that went with pure scientific work. Words like "do-gooder" and "tender-minded" often served to express and perpetuate the disparagement felt for those who were mainly interested in what psychology could do to help people and improve the human condition...It is hardly strange, under these circumstances, that serious conflicts have developed between "scientific" psychologists and "professional", especially clinical psychologists. (Tyler, 1973, p. 1021.)⁵

Hudson goes on to give a fascinating possible reason behind this "remarkable" insistence upon scientific status:

When a teaching department's projection of a professional identity is unusually insistent, one's impulse, whether or not one has truck with Jungian ideas, is to look for sources of professional anxiety. In the case of psychology these are not hard to find. Psychologists have a marginal position in the academic community, poised near the borderline between the humane and the scientific disciplines; we have a farouche professional past, redolent of mesmerism, even of witch-doctoring; and there still exist widespread misgivings--both in academic life and with society at large--with any attempt to examine the mind's contents. Our response, professionally, has been to over-react; to observe all the outward signs of scientific respectability, taking as our model, incidentally, the Victorian conception of the physical sciences, a model that physical scientists themselves have

⁵For another view of the same pattern: "Among British scientists, and with few exceptions, the pure look down on the applied, the physical, on the biological. And all continue to look down on the social, or "Mickey Mouse" scientists who are scarcely scientists at all... Psychology stands low in this pecking order, and contains a pecking order within it. Again, the pure look down on the applied, and the clean on the messy. The experimental, usually physical or biological in background, look down on the social, industrial, clinical, and educational. The psychologist of high status works in a laboratory, and studies either a sub-human species--rat, pigeon, monkey--or some simple aspect of human skill. The psychologist of low status works with human beings in their natural habitat, and studies them in their full complexity. The psychologist of high status works on problems that to the untutored eye seem trivial; the one of low status, on problems that laymen are more likely to understand. (Hudson, 1972, p. 53)

long abandoned. Here, as elsewhere, one has the impression of a professional group plunging, in search of an identity, from one extreme to another. A sense of orderly growth is lacking; so too is any awareness that urging propounded theses usually carry their own negation buried within them.(p. 54)

This disparity between what the novice wants to learn and what he/she is told is good and right for him/her to learn, remains and is reflected in the disparity of prestige between "scientists" and "practitioners"; it contributes to the sense of crisis regarding identity. After all what should clinical (or social, developmental, cognitive, or engineering) psychology be? What should "it" study? How? What could it study? Is "study" science?, etc. Sometimes involved in this complex of questions is a group questioning whether psychology is, has ever been, should be (could be) "pure" science, purity being juxtaposed here with either "applied science" or scholarship involving public policy.

There are some who cite an identity crisis (Viteles, 1972, p. 604) because of what they term the "uncertainty" as to whether the discipline at this stage is to be primarily "science or action; fact or fiction; cult or knowledge; a scholarly discipline or a medium for frequently premature application of views and methods of highly doubtful validity.." When Viteles states his position in certain terms ("service or research; community action or enhancement of knowledge; participation in movements or firming up the foundations of academe; the advancement of science or the construction of 'instant Utopia'") there is little doubt regarding his views on the scientist-practitioner model. Regarding his views as to the conduct of science, he comes down squarely for what is often

considered a "value-free" objective, Newtonian model as the way out of the woods. The tendency on the part of psychologists to confound speculation with scientific content, and to inject value judgments.

in a manner that makes it increasingly difficult, especially for the student and the layman, to determine when the psychologist is dealing with facts and principles derived from experiments, or when he is merely presenting his own value judgments. It has, in other words, become exceedingly difficult to know when the psychologist speaks with the authority of science, or when he is playing the role of the social reformer while clothed--or even disguised--in the garb of the scientist. (Viteles, 1972, p. 605)

He is essentially espousing a return to psychology's dominant model and philosophy of research as a procedure for extricating ourselves from our collective identity crisis. Hudson, on the other hand, (as might be expected by now), would disagree with Viteles's recommendations, as he has already stated that,

If we are to recover our pristine vigour, a major change is in store; not at the periphery, nor in detail, but at our corporate hub--a change in our conception of what we are about. And such a change must hinge on the emergence of a new model with which we can epitomize ourselves; a new root metaphor from which our more day-to-day activities will flow (Hudson, 1972, p. 157).

Operations in research philosophy and method. The identity crisis issue is related to divergences in research philosophy; what psychologists do in the daily practice of their discipline, determines who and what their identity as psychologists might be. This last major concern appears to be the most complicated and acrimonious. Cronbach (1975, p. 116) confesses some pessimism regarding psychology's predominant norms and procedures, then mentions the questioning of others (e.g., Gergen, 1973;

Glass, 1972; Israel and Tajfel, 1972; McGuire, 1973; Newell, 1972).

Methodological challenges. The first set of concerns regarding scientific method revolves about the appropriateness and efficacy of the current experimental model. The rules of statistical analysis and instrumentation have been challenged on methodological grounds. Signorelli (1974, p. 866) questions the position that psychology is evolving as a science, because it is increasingly using statistical analyses to evaluate its postulates. His point here is well taken, as it is usually those who are most enthusiastic in claiming progress because of statistical procedures, who also hearken to the physical sciences for their procedural models. Signorelli points out that the use of mathematical procedures in the development of the physical sciences bears little resemblance to the current use of statistics in psychology; moreover, instrumentation in the physical sciences is designed in accordance with, and to test, physical concepts, whereas in psychology, the situation is reversed, that is, instrumentation is heavily influenced by statistical concepts. For example,

The Skinner box is designed not to measure the presence of such theoretical factors as intensity of drive or reinforcement; rather, it is designed primarily to measure the frequency of the response. Intelligence tests and personality and diagnostic scales require the compilation of averages and correlative statistics to produce classifications and to demonstrate reliability and validity of the scale; the factor under measure is inferred from the power of the statistical results (Signorelli, 1974, p. 869).

In addition to statistically-based instrumentation and concepts, theory construction, and validation or falsification also appear to be

increasingly oriented to statistical formulation. Reliance on statistical significance, for instance, frequently obscures implications of experimental results, leading to controversies that are not resolvable at the level in which they are conducted. Signorelli (1974, pp. 867-868) reports an interesting study trying to decide between two formulations about the precise nature of reinforcement -- one based on response characteristics, the other on drive-reduction. A series of experiments attempting to demonstrate the superiority of one formulation over the other was inconclusive - significant results supported each formulation. Signorelli suggests that if the obvious is accepted--that the two formulations are complementary rather than mutually exclusive, neither further experimentation nor controversy would be necessary, while an integrating formulation is.

Statistics are also cited as increasingly becoming the basis around which theory is constructed as well as tested; Signorelli (1974, p. 866) highlights Estes' learning theory, Feigenbaum and Simon's formulation of the serial position curve and John's hypothesis regarding memory storage. It should be noted that while Signorelli particularly takes issue with experimental psychology, he absolves none of the sub-disciplines.

His points are reminiscent of those made by Koch that the development of psychology as a science "was unique in the extent to which its institutionalization preceded its content and its method preceded the problem." (Quoted by Gadlin and Ingle, 1975, p. 795). Gadlin and Ingle (1975, p. 795) state that they share Koch's contention that many of the

current problems in the discipline are a consequence of "method preceding content" and I would agree.

Paucity of theory. The second issue regarding scientific philosophy relates to the paucity of theory in the discipline. Elms (1975, p. 970) laments that theories have not fared well under empirical test and is of the opinion that any "would be" theorist today would find it difficult to propose any level of integrative theory in social psychology, with reasonable confidence in its longevity. Similarly, others (e.g., Bonneau, 1975, p. 800) are articulating the need for "major frameworks," or schema, to tie together and relate the various bits of information we have.

Besides worrying about the longevity of a proposed theory, it would be appropriate to worry about its reception and treatment. Theories in psychology are "critiqued" into oblivion. At times, the glee of the combat is all too evident. Why not, in fact, rather than refute, construct tests whereby hypotheses, as well as being capable of refutation or confirmation, can be confirmed only in such and such conditions, or can be made exceptions, inclusions or subsumptions? In short -- that X holds under these conditions, not under those. This approach, which would retain the confirmed material and integrate, if not "correct" the other, would have the virtues of continuity (in time and across subject matter) and less wasted time in re-discovery. Some interesting insights into why commonsense has, once again, not prevailed, are afforded by Hudson. It becomes apparent that the socialization process into psychology

fosters such an approach (1972, p. 103).

The education I myself received undoubtedly had the effect [Marcuse] predicts: I was equipped neither with the language, nor the concepts, nor the self-confidence, to phrase questions of a general kind. We were taught to dismantle, but not to reconstruct; the doctrines of the philosophers acting--as Bertrand Russell has said as a 'corrosive solvent' of the great systems of the past, yet putting nothing, beyond a mood of skeptical complacency, in their stead.

Another insight, this time into the perpetuation rather than origin of such a pattern, is forwarded by Elms (1975, p. 973); "The typical procedure...often seems more effective in producing professional publications than in locating and explaining important aspects of human social interaction."

Another exploration relates to psychology's uncertain scientific status. In the spirit of minority-group process, we can be more critical of our own theories than anyone else, before they are.

Suffice it to say, there is growing disillusionment with the experimental method as the dominant scientific procedure for the discipline.

As Pereboom has summarized (1971, p. 439):

The application of the experimental approach to a multi-dimensional discipline presupposes that it will work, that control and analysis will generate explanations which will lead to a unified theory for a restricted behavior domain, and that there will be a fundamental basis for our concepts, scales and methods which will justify the measurable generalization of that theory. This has not yet happened.

Objectivity and subject-object split. Experimental procedures have also been criticized on philosophical and ethical grounds as well as the procedural and consequential previously reviewed. Since they are con-

siderably more complex, philosophical issues will be just briefly reviewed in the present context. A primary area of criticism has been the traditional stance of objectivity and the resulting subject-object split in research. Because of the nearly total dominance of the experimental method and its philosophical context a number of consequences have gradually evolved. These include the assumption of independence between subject-matter and method (Gadlin and Ingle, 1975, p. 793), such that any variety of phenomena could be investigated by the same method, essentially without questions regarding the appropriateness of said method. Gadlin and Ingle (1975) have addressed themselves to several of the ramifications of such a meta-method. This includes, due to the emphasis on "objectivity", a split between experimenter and subject, such that the experimenter was purportedly "neutral" and the subject became object; that is, the subject necessarily became objectified as a manipulable entity, i.e., "thing."

This approach both denies the relational aspect of research (by objectification) and mitigates against its recognition. Gadlin and Ingle (1975, p. 796) suggest that the attempt to deny the relational aspect of research be abandoned and that it be actively attended to and investigated, such that,

the relationship between investigator and subject is overtly recognized as influencing the data. The relationship is seen as establishing a condition for the data to emerge, and examination of the relationship between investigator and subjects becomes part of the data analysis itself. (H. Raush, personal communication, quoted by Gadlin and Ingle, 1975, p. 796).

As such, the relational aspect of research would be regarded as necessity rather than unavoidable epiphenomenon or artefact.

A proposed model includes a research relationship in which "researchers and participants mutually explore psychological phenomena." (p. 796).

Additionally, reflexivity can be created by acknowledging that the study of human behavior necessarily includes the behavior of psychologists. This recognition implies, of course, that the psychologist is as prone to psychological processes as anyone else and should be especially self-conscious of this fact when acting as a scientist. This self-consciousness includes the psychologist's awareness of his relationship to and with his subject matter and the awareness of his own role with respect to his inquiry. The knowledge that derives from such reflexivity is a tripartite knowledge--about the subject, about the researcher, and about the knowledge itself. Little has been written about such matters in the psychological journals, but there does exist a small if obscure literature known as critical social science theory. The works of Habermas, Ratner, and Horkheimer can provide an introduction for those interested (Gadlin and Ingle, 1975, p. 796).

Obviously, an existing model embodying reflexivity of "investigator" and "subject" is found in psychotherapy as it is practiced by the relational therapists--for example, Rogerians, psychoanalytic therapists, and especially Sullivanians. H.S. Sullivan's articulation of the relational aspects of personality development, psychopathology and the therapist and researcher as participant-observers, remain paradigmatic for relational conceptualization and practice.

This is not to imply that all psychotherapies are necessarily relational or relationally-based; for instance, the theory, if not always the practice, of behavioral modifications, token economies, and rational-

emotive therapy are not. In fact, behavioral theory is explicitly based on the method and meta-method of the dominant research framework which is presently under criticism for its denial of relational aspects. The question of relational accountability echoes throughout the discipline, whether in research or therapy, and will be seen, in ensuing chapters, to be a focus of interest with regard to the crisis in psychology and attempts to use a Kuhnian classification for a social science.

Related to the issue of subject-object split are questions from a variety of perspectives about the validity of assuming that science is value-free, in its problem choices, methods, and the uses to which it is put. Buss (1975, p. 986) refers to a change from "voices in the dark" to "a growing army of psychologists who..."can no longer subscribe to the notion of a "pure" or "value-free" science.

Traditional view of scientific "producer" and "consumer". The traditional view of the relationship between scientific "producer" and "consumer" has also come under fire. Garner (1972, p. 942) describes, albeit facetiously, this relationship and terms it a fable.

There is a fable, carefully nurtured over the centuries by the basic scientists, particularly those who see basic as pure, about the relation between the scientist who acquires information and the problem solver who applies that information. The fable is that scientists acquire the knowledge, that this knowledge goes into the public domain, and that when a problem solver needs some knowledge to solve his problem, he extracts it from the public domain, uttering words of gratitude as he does so, and solves his problem. The actuality that the scientist has provided knowledge needed by the problem solver occurs in some mysterious fashion. Mysterious though the process is, it is so effective that no tampering

must be allowed, and in fact, the less contact the scientist has with the problems of the problem solver, the more apt he will be to fill the public domain with knowledge of ultimately greatest import to the problem solver. This is the fable, but...It does not work that way at all (Garner, 1972, p. 942).

Raush (1974) has addressed this line of concern in some depth, challenging the conventional wisdom that research is produced by scientists, the information from which filters "down" to practitioners, who used it and at times may tentatively make observations which serve as hypotheses to the scientific community. Raush (1974, p. 678) contends, rather, that "The consumers for formal, statistical psychological research and for the laboratory experiment are other researchers. An overview suggests that research clearly influences research...So far as one can see, again with the possible exception of behavior modification approaches,⁶ research has not influenced practice". [emphasis added]

Related to this issue is why there has been so little substantive knowledge and contribution from these well established traditional research lines. Raush (1974, p. 679), however, makes the interesting point that such substantive contributions have come from non-psychologists or from those psychologists "---and here I include not only Erikson and Rogers, but others like Fromm, Maslow, and May -- who have dissociated

⁶At another point, Raush notes that even among some of those well disposed to behavioral modification, questions have been raised as to the nature of the relation between operant research and behavior modification (1974, p. 678).

themselves from formal psychological research methods...Does formal research, as reported in our journals, offer little for application?"

This self-dissociation would appear to be a contributor to the failure of integration between practice and traditional psychological research methods. Another contributor would seem to be the status differential which invests the practitioner with lower prestige than the academic, scientific psychologist.

Raush's (1974, p. 679) suggestion is that,

it is not research that is being rejected, but one kind of research. The practitioner, whether as producer or consumer, rejects the traditional model of statistical research because it is of no value to him. The academic psychologist deplores this rejection and misinterprets it as a rejection of science itself. It is as though the rejection of a particular political platform meant that people did not want government.

The continuing rejection by practitioners of the traditional psychological research method is due to "fundamental inadequacies of those research methods for tackling issues open to the practitioner." (p. 681).

Essentially, Raush advocates a pluralism in officially legitimized research. While he would not eliminate traditional approaches, he is urging the legitimizing (through accrediting agencies and such institutions as journals and training programs) of alternate research approaches, designed specifically for the contingencies of psychological practice (p. 679). That is, models of research appropriate to the investigation of human and inter-human processes must be developed and legitimized as important in their own right, (and not as bastard children to "real

science" fit only for the service of broaching hypotheses, but not in succession with legitimate status in the scientific establishment). One such conceptualization of a different role for the psychologist in such a research process would be as a participant-conceptualizer, rather than scientist-practitioner, (a conceptualization suggested in 1965 at the Swampscott Conference [Raush, p. 680] and probably based upon H.S. Sullivan's concept of participant-observer.⁷ Interestingly, Sullivan was one of those who stood outside the traditional research approach, and as a practitioner, made significant clinical and scientific contributions, integrating the two and arguing explicitly for this view of science receiving full legitimacy.).

External validity. Questions regarding the limits of experimentally derived knowledge have also arisen, particularly with regard to whether people behave in markedly different ways in experimental situations than "outside" during their usual living (Gadlin and Ingle, 1975, p. 791). A related concern is whether experimental situations investigate, and of course measure, what is purportedly the subject of investigation. Similarly, the choice of subject matters has spiraled inward, under the necessity of meeting experimental conditions and controls, until the subject matter is largely trivial, or irrelevant. Hudson faults psychology

⁷Dr. Harold Jarman has pointed out that participant-observant approaches have multiple roots, including a substantial tradition in cultural anthropology and in 19th and early 20th century society. Sullivan's work, mentioned here, was a probable root in clinical psychiatry.

for the practice wherein we "lift people from the context in which they live and set them down in our departments and laboratories -- where we are at ease and they are not. Or we avoid the human race altogether, and settle for the monkey or the rat ..." (Hudson, 1972, pp. 152-153). He feels that, to a degree that is "astounding," academic psychologists have shunned not only actual contact with people, but the ideas about people that would evolve from such contact, preferring simple abstractions, in what he regards as the flight "from our historically constituted subject-matter." (pp. 151-152).

Repudiation of method seen as repudiation of science. With such a variety of criticisms, some of them fundamental to the research process, it is perhaps an object of wonder that the experimental method has not been abandoned, or at least transmuted. One such reason (among many fully as significant) is that abandoning the experimental model means a good deal more than abandoning merely a method (Gadlin and Ingle, 1975, p. 793); it would be essentially abandoning science itself as it is currently practiced and construed. The experiment is virtually synonymous with good scientific practice and has been the overwhelmingly dominant, and successful, form of science to date.⁸ To challenge it as

⁸Gadlin and Ingle (1975, p. 793) have stated that: "Abandoning the experiment would be much more than the abandonment of a prevailing method; it would be desertion of a paradigm." From their import, they apparently refer to an orientation toward practice as well as that practice itself. While the present author would agree with their contention, and obviously has, it is well to point out that their use of "paradigm" here is no longer appropriate, as will be made clear in the next

a method carries the direct implication of challenge to the dominant practice in science, and practically, to science itself. The significance of the situation is compounded by the fact that, since this method is construed as science itself, there is no way in terms of methodology, to stand outside the practice to critique and change it; and with its dominant philosophy of science eschewing such critique and historically addressing questions of method, while assuming precisely the present areas of criticism (Gadlin and Ingle, 1975) the crisis is very real, and perhaps fundamental.

A number of people have dealt with their concern on just such a fundamental level. Tyler (1973, p. 1024) contends that we require both new guidelines for research and "some new models for research, not just adapted from physics or biology, but created especially for the sciences in which scientist, subject, and consumer all belong to the same species." The disillusionment with the traditional framework is thoroughgoing (see Hudson, 1972, pp. 11 and 75), as is the repudiation of a philosophy of science that has engendered a form of science termed futile (Signorelli, 1974, p. 869). The intensive exploration of alternate methodologies has been recommended (Raush, 1974; Gadlin and Ingle, 1975), rather than the "simple" abandonment of the experimental method, which would indeed be tantamount to "scientific suicide" (as it is essentially synonymous

chapter regarding the schema proposed by Thomas Kuhn about the development of science. It will also be apparent that most people using the paradigm term have fallen prey to this problem, helped along in no small measure by Kuhn's acknowledged inconsistencies with regard to the term.

with science). Some feel that psychology is in an era of Kuhnian revolution (Palermo, 1971, p. 136) and others are calling for a new Kuhnian paradigm -- in this sense, a new framework for scientific theory and practice (Elms, 1975; Gadlin and Ingle, 1975).

It is apparent that the crisis in psychology has emerged from a number of contexts and is by now challenging the fundamentals of scientific conceptualization and practice. The criticisms have been of essentially two types, the first by psychologists (and others in allied fields) who are making methodological criticisms from within the same framework as the research they are addressing. The second type of critique poses a more serious crisis; this critique is by individuals who have stood, or are beginning to stand, outside the framework of the experimental method. They are critiquing not only the method, but the assumptions, premises, goals, and philosophy of the dominant scientific framework. It is probable that at present with the depth and extent of the present crisis, the differences between competing frameworks will begin to be articulated on a disciplinary level, and the controversy (regarding both method and framework) will increase.

As a discipline, psychology has not been averse to fervent and often chronic controversy; this history of acrimonious and usually fruitless controversy bears a direct relationship to the lack of general recognition that psychologists do not always share the same framework, and not want the same things.

At Oxford, we were initiated into the joke about the introspective psychologists who, in the early years of

the century, fell into furious debate over whether green was in truth a yellowish blue, or a blueish yellow. The example is trivial, but the epistemological difficulties such a story parodies are real enough, because the science in which no two scientists can agree on the evidence is no science at all (Hudson, 1972, p. 144).

Not only can psychologists not agree on the evidence, we usually cannot agree on the questions.

Presence of sterile controversy. While psychology as a discipline has had a history of active controversy, by no means have all of these controversies proved productive. In fact, many of them have been characterized as evangelical rather than scientific in that people regard their arguments as directed towards ways of life rather than methods of doing science (Sutherland, 1973). Still other controversies appear no closer to resolution than they were when introduced scientific generations ago, and in fact, though continuing in one variant or another, no longer shed new light on the issues and may be termed "sterile." If the controversialists were not so intensely committed to their views, the debates would by now sound quite stale. As it is, with some controversies, it is quite difficult to ascertain what their role can be in the development of psychology.

In view of this history of active controversy, and in the context of a felt crisis where authors differ about preferable solutions to this crisis, it may help to see if there are consistent characteristics which distinguish successful from unsuccessful controversies in research and practice. What are the characteristics of those controversies that

have been resolved or appear to be progressing to resolution? What are the characteristics of controversies that, though not being resolved seem at least to shed light on the areas of debate? Can some characteristics of the perpetual, unresolved, i.e., "sterile" debates be identified? Are any particular types of controversy especially facilitating or impeding in the development of psychology?

C H A P T E R I I

KUHNIAN CONCEPTUAL SCHEMA, REVISIONS AND COMMENTS

Early Formulations and Reception

Kuhn's early formulation of paradigm, revolution, and normal science met an interesting disparity of reactions: on the one hand enthusiasm and application by scientists (Grinker, 1967; Stierlin, 1977; von Bertalanffy, 1968), and on the other, extensive criticism by philosophers (see Shapere, 1964; and especially Lakatos and Musgrave, 1970). One of these philosophers, Masterman (1970, p. 59-60), draws attention to this and offers a possible origin for this disparity.

It is being widely read, and increasingly appreciated, by actual research workers in the sciences, so that it must be (to a certain extent) scientifically perspicuous. On the other hand, it is being given widely diverse interpretations by philosophers, which give some reason to think that it is philosophically obscure. The reason for this double reaction, in my view, derives from the fact that Kuhn looked at actual science, several fields, instead of confining his field or reading to that of the history and philosophy of science, i.e. to one field. Insofar, therefore, as his material is recognizable and familiar to actual scientists, they find his thinking about it easy to understand. Insofar as the same material is strange and unfamiliar to philosophers of science, they find any thinking that is based on it opaque.

Kuhn's (1970a, p. 271) responses to most of the philosophical critique indicate that he was convinced his position had been largely misunderstood and/or distorted. For instance,

It is now four years since Professor Watkins and I exchanged mutually impenetrable views ... rereading our contributions together with those that have accreted to them, I am tempted to posit the existence of two Thomas Kuhns, Kuhn₁ is the

author of this essay and of an earlier piece in this volume. He also published in 1962 a book called The Structure of Scientific Revolutions, the one which he and Miss Masterman discuss above. Kuhn₂ is the author of another book with the same title. It is the one here cited repeatedly by Sir Karl Popper as well as by Professors Feyerabend, Lakatos, Toulmin, and Watkins. That both books bear the same title cannot be altogether accidental, for the views they present often overlap and are, in any case, expressed in the same words. But their central concerns are, I conclude, usually very different. As reported by his critics (his original has unfortunately been unavailable to me), Kuhn₂ seems on occasion to make points that subvert essential aspects of the position outlined by his namesake.

Kuhn attributes this mutual impenetrability to the kind of "Gestalt-switch" he discusses in scientific revolutions and regards the misunderstandings as "an extended example of what [he has] elsewhere called partial or incomplete communication -- the talking-through-each-other that regularly characterizes discourse between participants in incommensurable points of view" (pp. 231-232).

Kuhn's position was that, since most of his critics had not understood his schema, their critiques were largely irrelevant.

Masterman's critique was the exception. It is significant, I think, that Kuhn uses Masterman's criticism so thoroughly. He at no point accuses Masterman of distorting his material, acquiesces with most of the criticisms and goes on to base his revisions on them. His reason for this seemed to be his perception that Masterman's critique was the only one that understood his schema, making that Gestalt-switch he emphasizes. He pointed out (1970a, p. 234) that they approached the problem (that is the necessary revision of the paradigm concept), in the same spirit,

though, "my present position differs from hers in many details."

Masterman does indeed take the position of sympathetic critic which is perhaps attributable to the stand she explicitly takes as a "working scientist" rather than philosopher. It was Masterman, in fact, who first pointed out the very different receptions accorded Kuhn's work by scientists and philosophers. Masterman has no trouble understanding Kuhn's concept of normal science, regards his paradigm idea as scientifically useful, and perceives its current use by scientists. She also manages to take a swipe at two of Kuhn's more notable detractors. As Masterman states (1970, p. 60-61),

That there is normal science -- and that is exactly as Kuhn says it is -- is the outstanding, the crashingly obvious fact which confronts and hits any philosophers of science who set out, in a practical or technological manner, to do any scientific research. It is because Kuhn -- at last -- has noticed the central fact about all real science .. mainly that it is a normally habit-governed, puzzle-solving activity, not a fundamentally upheaving or falsifying activity (not, in other words, a philosophical activity), but actual scientists are now, increasingly reading Kuhn instead of Popper; to such an extent indeed that, in the new scientific field particularly, 'paradigm' and not 'hypothesis' is now the 'okay word.' It is thus scientifically urgent, as well as philosophically important, to try to find out what a Kuhnian paradigm is. Since my overall viewpoint is scientific, this paper also measures that science as it is actually done, i.e., science roughly as Kuhn describes it -- is also science as it ought to be done .. For the one thing working scientists are not going to do is change their ways of thinking, in doing science, ex more philosophico, because they have Popper and Feyerabend pontificating at them like eighteenth-century divines; particularly as both Popper and Feyerabend normally pontificate at even more than eighteenth-century length...

This preface is, I fear, a shade aggressive; compression of material and indignation with what I shall call in the paper 'philosophy-of-science-aetherialism' have caused this. In any case, in view especially of some of the more interesting

phrases used by Watkins, a little pro-Kuhn aggressiveness injected into the symposium will not do any harm.

Lest we begin to think Masterman an uncritical sympathizer, it should be noted that she explicitly agrees with Kuhn in one major area, that of paradigm concept, but disagrees with him in another major area, i.e., verification (1970, p. 61) and twice takes him to task for lack of clarity: first, for confusion about multiple-paradigm science and disregard of the role of technology in science, and secondly, for his posing two incompatible solutions for his inability to find the rules for puzzle-solving.

Masterman's Critique

Masterman begins her critique by raising a most interesting point -- "... it is curious, that, up to now, no attempt has been made to elucidate this notion of paradigm, which is central to Kuhn's whole view of science" (1970, p. 59). When she examines his 1962 usage of the term, she found that Kuhn used 'paradigm' "in not less than twenty-one different senses ... possibly more, not less" (1970, p. 61). Textual analysis concerned with possible commonalities, with anything definite or general about the paradigm idea, indicated that Kuhn's twenty-one usages of paradigm fall into three large groups. And this is the heart of paradigm revision (1970, p. 61).

For when he equates "paradigm" with a set of beliefs (p. 4) with a myth (p. 2), with a successful metaphysical speculation (p. 15), with a standard (p. 102), with a new way of seeing (pp. 117-121), with an organizing principle governing perception itself (p. 120), with a map (p. 108), and with something

which determines a large area of reality (p. 128), it is clearly a metaphysical notion or entity, rather than a scientific one, which he has in his mind. I shall therefore call paradigms of this philosophical sort metaphysical paradigms, or metaparadigms; and these are the only kind of paradigm which, to my knowledge, Kuhn's philosophical critics have referred. Kuhn's second main sense of "paradigm" however, which is given by another group of uses, is a sociological sense. Thus he defines paradigm as a universally recognized scientific achievement (p. X) as a concrete scientific achievement (pp. 10-11) as like a set of political institutions (p. 91), and as like also to an accepted judicial decision (p. 23). I shall call paradigms of this sociological sort sociological paradigms. Finally, Kuhn uses the "paradigm" in a more concrete way still, as an actual textbook for classical work (p. 10), as supplying tools, (pp. 37 and 76), as actual instrumentation (pp. 58-60); more linguistically, as a grammatical paradigm (p. 23), illustratively, as an analogy (p.g. on p. 14); and more psychologically, as a Gestalt-figure and as an anomalous pack of cards (pp. 63 and 85). I shall call paradigms of this last sort artefact paradigms or construct paradigms.

Of these three major types, Masterman chooses the third, the artefact or construct paradigm, as the fundamental sense of the concept, what she calls the "initial practical trick-which-works-sufficiently-for-the-choice-of-it-to-embody-a-potential-insight" (1970, p. 70). It is the concrete problem solution, the new way of seeing the problem which allows it to be solved and which can then be extended to other similar phenomena. The construct or artefact paradigm is a concrete picture that is then used analogically. The paradigm then has two functions: one which solves the original problem and the other "that being a picture of one thing ... is used to represent another -- for example, a geometric model made of wire and beads, though it is primarily a glorification of a well known kind of child's toy, is used in science to

represent a protein molecule" (1970, pp. 76-77). The construct or artefact paradigm antedates the research program and theory, it is the way of seeing something, the flash of insight, that cracks the problem. Often, this paradigm is supplied by what Masterman refers to as "some rank outsiders with a quite different viewpoint and rudimentary techniques [who] succeed." (1970, p. 84), in large part because of their different perspectives and the crudeness of their technique. Masterman considers most paradigms to be relatively crude, to have a basic quality of concreteness (1970, p. 67). The paradigms are nothing if not practical, usable, and rather primitive. They are unelaborated, unrefined, but rather inspired insights.

Paradigm Revised

Kuhn's revision of the paradigm concept relies heavily on Masterman's analysis and recommendation though he used a somewhat different structure. Masterman assigned the 21 paradigm senses to three categories, the construct or artefact, the sociological and metaphysical paradigm. She then identified the artefact paradigm as the meaning she thought was fundamental to Kuhn's schema.¹

¹It seems to me that Masterman is correct but for reasons other than those she forwarded. She makes a case for the artefact sense being fundamental because: if a paradigm must come before normal science, if normal science is a fundamentally "puzzle-solving" activity, and if artefacts solve puzzles, then the paradigms needed to solve the puzzle are the artefact paradigms. Her argument can be damaged by terming normal science a "problem-solving" venture, although I do prefer "puzzle-solving" for its connotations. But it does seem to me that this argument of Masterman's

Kuhn splits his original broad usage of paradigm into two inter-related concepts: a construct or artefact paradigm and a disciplinary matrix (DM).² He thus retained Masterman's artefact paradigm as his primary conceptualization of the term and assigned all the previous meanings of the paradigm to the DM category. "All of the objects of commitment described in my Scientific Revolution as paradigms, parts of paradigms, or paradigmatic would find a place in the disciplinary matrix, but they would not be lumped together as paradigms, individually or collectively (1970a, p. 271). The DM defines a community and provides those constructs and methods that "enable [adherents] to solve puzzles and that accounted for their relative unanimity in problem choice and in the evaluation of problem solutions" (1970a, p. 271). As Kuhn points out, the DM is essentially Masterman's social paradigm.

To explicate the revised paradigm concept first; retaining Masterman's artefact or construct paradigm, Kuhn re-defined his paradigm as "exemplar" (1970a, p. 271): "a universally recognized scientific achievement that for a time provides model problems and solutions to a community

is not nearly so solid as her points regarding the three categories, the priority of paradigm to theory (66) and to the sociological paradigm (69-70), and the artefact paradigms used as a way of seeing (73), analogically (77), from the original picture solution to the operational reinterpretation. While she seems to like the play on words about puzzle, I am more impressed by other of her arguments.

²"Disciplinary because it is common to the practitioners of a specific discipline, and 'matrix' because it consists of ordered elements which require individual specification." (Kuhn, 1970a, p.271).

of practitioners" (1970b: viii). Paradigms are concrete problem solutions and at least some of the very early technical problems solutions (1970b, p. 18). As Masterman points out, Kuhn assigns the central place "in real science, to a concrete achievement" rather than to "an abstract theory" (1970a, p. 66). Kuhn and Masterman point out here and there that these achievements must have three more characteristics to function as paradigms, as opposed to problem solutions only. They must be "sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity" and they must be sufficiently open-ended to leave all sorts of problems for the re-defined group of practitioners to solve. Without these two requirements, the paradigm could not exist, as a paradigm must point the way while attracting followers, otherwise there would be no identifiable group with which it is associated, nor, any subsequent line of investigation which articulates it as a paradigm.

Finally, to function as a paradigm, a concrete problem solution must be usable as an analogue; it must provide a Gestalt with which to "see" new problems as subjects for the application of similar techniques.³ This enables adherents to see some problems as "like each other" and therefore apply an interpretation of the model solution. It would appear

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This dual aspect of paradigm, as simultaneously solution and exemplar, occasionally causes trouble. For instance, "...Pavlovian conditioning is limited as a paradigm-spawning exemplar for two reasons." (Lipsey, 1974, p. 408). Exemplars do not "spawn" paradigms, they are paradigms.

that this process simultaneously: 1) solves the new problem, 2) extends the range of the paradigm's applicability; and 3) demonstrates the efficacy of that paradigm. Kuhn provides a very nice example of this dual function of solution and analogy, in Galileo's solution of the ball rolling down an inclined plane and the subsequent use of this solution as an analogue.

Galileo found that a ball rolling down an incline acquires just enough velocity to return it to the same vertical height on a second incline of any slope, and he learned to see that experimental situation as like the pendulum with a point-mass for a bob. Huyghens then solved the problem of the center of oscillation of a physical pendulum by imagining that the extended body of the latter was composed of Galilean point-pendula, the bobs between which could be released at any point in the swing. After the bobs were released, the individual point-pendula would swing freely, but their collective center of gravity when each was at its highest point, would be only at the height from which the center of gravity of the extended pendulum had begun to fall. Finally, Daniel Bernoulli, still with no aid from Newton's Laws, discovered how to make the flow of water from an orifice in a storage tank resemble Huyghens' pendulum, determined the descent of the center of gravity of the water and tank and jet during an infinitesimal period of time. Next imagine that each particle of water afterwards moves separately upward to the maximum height obtainable with the velocity it possessed at the end of the interval of descent. The ascent of the center of gravity of the separate particles must then equal the descent of the center of gravity of the water in tank and jet. From that view of the problem the long sought speed of efflux followed at once. These examples display what Miss Masterman has in mind when she speaks of a paradigm as fundamentally an artefact which transforms problems to puzzles and enables them to be solved even in the absence of an adequate body of theory.

Is it clear that we are back to language and its attachments to nature? Only one law was used in all of the proceeding examples; known as the Principle of vis viva, it was generally stated as 'Actual descent equals potential ascent.' (1970a, pp. 273-274).

Besides demonstrating the solution-analogue functions of a paradigm, Kuhn's example also speaks to the function of a paradigm in the absence of theory, a point which causes difficulty for some of his readers.

Kuhn remarks that physicists share few rules by which they make that transition from the Gestalt to the specific form of it demanded by the individual problem, and, instead, exposure to a series of exemplary problem-solutions teaches them to "see" different physical situations as like each other. Once a number of problem situations are seen this way, the individual "can write down ad lib" the interpretation of the Gestalt required by the characteristics of the particular new problem. Participating in this way of seeing teaches the individual what the words mean and how they "... attach to nature; equally, it is part of learning how the world behaves. The two cannot be separated..." (1970a, p. 274). The acquisition of a new paradigm as insight and method seems also to be the acquisition of a way of seeing "how the world is." Thus, "facts" about some phenomenon in one paradigm would be different than the "facts" about that phenomenon as construed by another paradigm.

The Disciplinary Matrix

Differentiation from paradigm. Kuhn's 1962 Structure of Scientific Revolutions obviously combined the paradigm and DM concepts. Thus, Kuhn cites as paradigmatic, several works which served for a time to implicitly define the legitimate problems and methods of a research field for succeeding generations of practitioners. He includes Aristotle's Physica, Ptolemy's Almagest, Newton's Principia and Opticks, Franklin's Electricity,

Lavoisier's Chemistry and Lyell's Geography (1970b, p. 10).

They were able to do so because they were both sufficiently unprecedented to attract adherents and sufficiently open-ended to leave problems for solution. But then Kuhn continues:

[These] accepted examples of actual scientific practice -- examples which include law, theory, application, and instrumentation together -- provide models from which spring particular coherent traditions of scientific research. These are the traditions which the historian describes under such rubrics as 'Ptolemaic astronomy' (or 'Copernican'), 'Aristotelian dynamics' (or 'Newtonian'), 'corpuscular optics' (or 'wave optics'), and so on (Kuhn, 1970b, p. 10).

The "coherent research traditions" should not be seen as identical to the technical problems solutions. The latter are the paradigms (e.g., the Almagest, or the Principia) which precede, serve as exemplars for, and from which are derived, the coherent traditions. These traditions are what Kuhn later termed disciplinary matrices (DMs) with characteristics, origins and functions all their own. This 1962 presentation in quotations indicates the point at which the 1970 revision separates the disciplinary matrix concept from the paradigm.

In separating the DM concept from the paradigm concept, Kuhn(1970a, p. 271) asked: "what its members shared that enabled them to solve puzzles and that accounted for their relative unanimity in problem choice and in the evaluation of problem-solutions?" Here he is essentially conceptualizing the DM as a constellation of group commitments (1970b, p. 181). It is a strong network of such commitments "conceptual, theoretical, and methodological" (Kuhn, 1970b, p. 42), which defines a community (Kuhn, 1970b, p. 177). This is the perspective within which Kuhn feels the DM

is what makes it possible for the members of a coherent tradition of research to have relative fullness in their professional communication with each other, and relative unanimity in their professional judgments (1970b, p. 182). It is important to recognize that a DM governs the practitioners and not the subject matter (Kuhn, 1970b, p. 180). Seen sociologically, the DM is "a set of scientific habits...[which] may be intellectual, verbal, behavioral, mechanical, technological" (Masterman, 1970, p. 66).

Internal structure. When addressing the disciplinary matrix "from the outside," trying to see what it does, what functions it serves, Kuhn seems more successful than when he tries to articulate a DM's internal structure. Here, when talking about internal structure, he pares the DM down to four constituents: paradigm or exemplar, shared symbolic generalizations, shared models whether heuristic or metaphysical, and shared values.

A DM is much broader than, and derives from, a paradigm. Kuhn is quite clear that the concrete scientific achievement must not be identified with, but must be seen as "prior to the various concepts, laws, theories, the points of view that may be extracted from it" (Kuhn, 1970b, p. 11). The paradigm is the concrete problem-solution used as analogue whereas the disciplinary matrix is the line of investigation founded upon that paradigm or paradigm-set which bind the practitioners.

Symbolic generalizations, another DM component, are those expressions deployed without question or dissent by group members, which can be

readily cast into logical form (1970b, p. 182). Sometimes these can be expressed in symbolic form, as in: $f = ma$ (force is equal to mass X acceleration). Other symbolic generalizations are more easily expressed with words such as "action equals reaction." These symbolic generalizations allow group members to attach their logical and mathematical manipulations to consensual sign posts, as well as functioning as laws and definitions for the symbols used. The symbolic generalizations are developed as the paradigm is articulated and are consensually validated within the DM.

The third major component of any DM is the shared belief in particular models "whether metaphysical, like atomism, or heuristic like the hydrodynamic model of the electric circuit" (Kuhn, 1970a, pp. 271-272). The heuristic model for molecules of gas regards them as behaving like tiny elastic billiard balls in random motion. The metaphysical models portray phenomena in such a way as to indicate what types of approaches are permissible. For example, with regard to metaphysical models as is pointed out in Marx and Hillix (1963, p. 180).

...there are two extreme views of the physical world and the role of systems in it. One view is that the world is composed of independent additive parts whose total constitutes reality. The other view is that everything is related to everything else, and there are no independent systems. The Gestaltists held neither of these extreme views, although they leaned toward the latter.

The metaphysical paradigms as presented by Kuhn address both epistemological and ontological concerns.

Finally the last component Kuhn discusses at any length (and that is

not much) are values. Kuhn feels that values are more widely shared among different communities than either symbolic generalizations or models, and that they do much toward providing a sense of community to natural scientists as a whole. Probably the most deeply held rules concern predictions: they should be accurate, quantitative measures are preferable to qualitative, etc. There are also values to be used in judging whole theories, e.g., theories should permit puzzle formulation and solution, it should be as simple as possible, self-consistent, and plausible (Kuhn, 1970b, p. 185). The issue of value seems to me poorly articulated; for instance, if values are more widely shared among different communities than the other DM constituents, and if they do more toward providing a sense of community to natural scientists as a whole, it seems as though in some ways values function outside the DM as well as inside the DM. How else could they provide a sense of community across DM's if these values did not, in some sense, also operate outside DMs? This is an area which deserves more scrutiny. It is also obvious that when Kuhn refers to values, he is using the term in the narrow sense, in relation to theory choice and construction.

After listing and discussing rather briefly the four components of the DM, Kuhn states, "...and other elements of the sort" (Kuhn, 1970a, p. 272). These "...other elements of the sort" while most certainly not elucidated by Kuhn here in his second approach (the internal structure of the DM), are included in the first approach (the function of the DM)--that is what it seems to do "for" practitioners. These mentioned, but

not developed, other components then, must include "scientific habits" in theory, method, and instrumentation/technology. All of which above have been mentioned as part and parcel of the DM by Kuhn and Masterman as they initially approached the DM concept. They then presumably included these concerns only under "other elements of the sort."

It is obvious, however, that they are necessary and should be articulated as to function and relationship with components of the DM. Kuhn places both paradigm and some aspects of disciplinary matrix prior to theory. For instance (Kuhn, 1970a, p. 271)

When I speak of knowledge embedded in terms and phrases learned by some non-linguistic process like ostension, I am making the same point that my book aimed to make by repeated reference to the role of paradigms as concrete problem solutions, the exemplary objects of an ostension. When I speak of that knowledge as consequential for science and for theory-construction, I am identifying what Miss Masterman underscores about paradigms by saying that they 'can function when the theory is not there.'

His points regarding paradigm solutions as exemplary objects of ostension, and as operating prior to theory are nicely spelled out in the previously presented quotation regarding Galileo, Huyghens, et al., using the paradigm in the absence of Newtonian theory, but with the picture-insight to guide them. Masterman (1970, p. 66) makes the further point that, by assigning the central place in actual science to concrete achievements, that is, the paradigm rather than the theory, "Kuhn, alone among philosophers of science, puts himself in a position to dispel the worry which so besets the working scientist confronted for the first time with professional philosophy of science, 'How can I be using a theory which isn't

there'?"

Relation Between Paradigm and DM

It is also clear that paradigms must initially precede DMs, since the paradigm provides the initial insight, and the normal science which articulates that paradigm must clearly exist subsequent to, and not prior to, the paradigm. But what does the scientist use in terms of method or perspective to develop a paradigm? Given the emphasis Kuhn places on technical problem solutions, the individual scientist might well be "importing" a technique or approach from a different DM. The insights or techniques of the normal science of a different DM are adapted and brought to bear on the problem at hand. In fact, Masterman alludes to this process when she talks about a paradigm being constructed by "rank outsiders" in a different field who come up with a crude, but useful, paradigm. (As we will see, this is precisely what occurred in the construction of the double bind paradigm.) If the technique, or "trick," is successful, the individual has a problem solution, and if that problem solution meets the criteria of unprecedented success, openness, and possibility of analogic use as Gestalt or exemplar, s/he also has a new paradigm; at this point, the "imported" technique, or "trick," if it remains useful, becomes the starting point and part of the new DM, and will become translated and modified through time to meet the requirements of the problems encountered. Those techniques or tricks not an aid in constructing the paradigm, or subsequently articulating it,

are obviously not part of the new DM.

During the early elaboration and articulation of the paradigm by DM adherents, the sequence is linear; that is, the paradigm exists, and being elaborated, forms a DM through normal science activity and the flow of "events" is from paradigms to DM. (Actually, this is necessarily the case, as the paradigm precedes the DM in time and function). As both components become developed, however, the linear quality changes; DM components reflect "back" onto the original paradigm, necessitating modifications. For instance, some refinement of instrumentation may require a change of paradigm quantification values (if the paradigm has been expressed in numbers), or, the elaboration of a theory might highlight necessary reformulation of a basic process in the paradigm. It is supposed that if paradigms are constructed in such a manner that makes modification impossible, the DM must eventually wither; whether this has actually occurred seems an interesting, and important point, though one which is tangential to present purposes.

Kuhn does not address himself to this loss of linearity, and thereby unrealistically limits his schema. The concept of "reflexivity" as developed by the ethnomethodologists (Mehan and Wood, 1975) appears to describe the actual relationship of the paradigm and DM, after the initial development of the DM. That is, both the paradigm and DM become defined by the adherents with respect to each other and as each becomes defined, the definition serves a defensive or confirming purpose for the other. In such a way the adherents maintain some "purity" of definition

and some overall cohesiveness in the DM.

Similarly, the use of new instrumentation as well as old techniques and methods may also allow new problems and solutions to emerge, e.g., as Kuhn points out, use of the cathode ray made the discovery of x-rays possible. The cathode ray and its use were instrumentation and methodology respectively but the solution to the problem remains the insight, that is the paradigm, not the instrument. At times, the two have been confused; for instance the concept of "instrumental paradigms" such as the shuttle box and rotary pursuit apparatus have been forwarded (Weimer and Palermo, 1973, p. 242, their footnote M). While Weimer and Palermo indicate that these devices function within the paradigm and meet exemplar requirements, it seems that they confuse the issue somewhat. These instruments may well be a part of a problem-solution that also serves as an analogue, and thus in part constitute a paradigm, but it appears unnecessary and potentially confusing to refer to them as "instrumental paradigms."

In a similar point, Lipsey (1974, pp. 407-408) takes them to task for arguing that behavioristic methods and techniques were exemplars, Lipsey's point is that they are not the same as a concrete problem-solution "resulting from the use of a particular method or technique." This point seems equivocal to me. For instance, if a familiar technique solves a problem, this seems to me to be normal science and therefore not the creation of a paradigm. If, on the other hand, a familiar method or technique is adapted to solve a new problem such that the solution

meets paradigm requirements, that adapted method-and-solution combination appear to be the relevant unit, and then, the method does indeed partially constitute the paradigm.

Functions of DMs

Gatekeeping. Kuhn considered some of the other important functions (beyond guidance functions previously discussed) of the DM. The first of these, essentially a gatekeeping function, involves the achievement of unanimity regarding the problem-choice and solution.

...One of the things a scientific community acquires with a paradigm is a criterion for choosing problems that, while the paradigm is taken for granted, it can be assumed to have solutions. To a great extent these are the only problems that the community will admit as scientific or encourage its members to undertake. Other problems, including many that had previously been standard, are rejected as metaphysical, as the concern of another discipline, as sometimes just too problematic to be worth the time (Kuhn, 1962, p. 37, cited by Masterman, 1970, pp. 82-83, footnote).

Weimer and Palermo (1973, pp. 223-224) give an exposition of a DM development, particularly with respect to its gatekeeping functions and institutionalization.

In sum, the paradigmatic and normal science nature of structural psychology is evident from a number of its distinctive characteristics. It specified a rigorous subject matter (consciousness and its contents) and a rigorous method (selbstbeobachtung) such that anyone studying another subject or employing another method was automatically not doing 'experimental psychology.' Laboratories were founded for the experimental study of the subject matter, while universities offered courses in the 'New Psychology', and departments gradually appeared as separate entities from physiology and philosophy. Professional initiates were

trained in these departments and laboratories. Professional journals, such as the Philosophische Studien (founded in 1881), appeared for the dissemination of research findings. Likewise, psychological professional societies developed for the purpose of fostering communication within the group. A certain amount of 'brass instrument' equipment, specific to the psychological laboratory, was developed. Despite the many conceptual and theoretical issues taken for granted (e.g., the utility and validity of studying the contents of the adult mind), there were numerous 'within the ring' (to use Titchener's turn of phrase) controversies concerning specific issues of fundamental theoretical if not paradigmatic import (such as whether the Ausfragemethode was a legitimate form of introspection). Throughout all of this, some basic metaphysical directives, such as 'Associationism is the mechanism of the mind', were endorsed unquestioningly. (Weimer and Palermo, 1973, pp. 223-224).

Communication and consensus. Kuhn also considers the communicational function of a disciplinary matrix; this appears to be a major function with respect to conducting normal science activities, to the maintenance of research communities, and to the perception and conceptualization of phenomena and decisions about how they should be researched.

One of the things upon which the practice of normal science depends is a learned ability to group objects and situations into similarity classes which are primitive in the sense that the grouping is done without the answer to the question, 'similar with respect to what?' One aspect of every revolution is then that some of these similarity relations change. Objects which were grouped in the same set before were grouped in different sets afterwards and visa versa. Think of the sun, moon, Mars, and Earth before and after Copernicus; of free fall, pendular, and planetary motion before and after Galileo, or of salts, alloys, and a sulphur-iron filing mix before and after Bolton. Since most objects within even the altered sets continue to be grouped together, the names of the sets are generally preserved. Nevertheless, the transfer of a subset can critically affect the network of interrelations among the sets. Transferring the metals from the set of compounds to the set of elements was part of a new theory of combustion, of acidity, and of the difference between physical

and chemical combination. In short order, these changes had spread through all of chemistry. When such a redistribution of objects among similarity sets occurs, two men whose discourse had proceeded for some time with apparently full understanding may suddenly find themselves responding to the same stimulus with incompatible descriptions or generalizations. Just because neither can then say "I use the word element (or mixture, or planet, or unconstrained emotion) in ways governed by such criteria", the source of the breakdown in their communication may be extraordinarily difficult to isolate and by-pass. (Kuhn, 1970a, pp. 275-276) [emphasis added]

Without insisting that there is no recourse in such situations, Kuhn does emphasize that these differences are very deep, not merely about definition, names or theory, but "equally and inseparably about nature. We cannot say with any assurance that the two men even see the same thing, possess the same data, but identify or interpret it differently" (Kuhn, 1970a, p. 276).

Kuhn's work here, it seems to me, is quite important with regard to controversy in psychology and so I will continue at some length.

The source of communication breakdowns now being considered are likely evidence that the men involved are processing certain stimuli differently, receiving different data from them, seeing different things or the same things differently... Nevertheless, there must be recourse. Though they have no direct access to it, the stimuli to which the participants in a communication breakdown respond are, under pain of solipsism, the same. So is their general neural apparatus, however different the program. Furthermore, except in small, if all-important areas of experience, programming must be the same, for the men involved share a history (except the immediate past), a language, and an everyday world, and most of a scientific one. Given what they share, they can find out much about how they differ. At least they can do so if they have sufficient will, patience, and tolerance of threatening ambiguity, characteristics which, in matters of this sort, cannot be taken for granted. Indeed, the sorts of therapeutic efforts to which I now turn are rarely carried far by scientists.

First and foremost, men experiencing communication breakdown can discover by experiment--sometimes by thought-experiment, arm-chair science--the area in which it occurs. Often the linguistic center of the difficulty will involve a set of terms, like element and compound, which both men deploy unproblematically but which it can now be seen they attach to nature in different ways. For each, these are terms in a basic vocabulary, at least in the sense that their normal intra-group use elicits no discussion, request for explication, or disagreement. Having discovered, however, that for intra-group discussion, these words are the locus of special difficulties, our men may resort to their shared everyday vocabularies in a further attempt to elucidate their troubles. Each may, that is, try to discover what the other would see and say when presented with a stimulus to which his visual and verbal responses would be different. With time and skill, they may become very good predictors of each others' behavior, something that the historian regularly learns to do (or should) when dealing with older scientific theories. (Kuhn, 1970a, pp. 276-277; emphasis added)

It is important to emphasize here Kuhn's points regarding differences in perception as well as meaning. Within one DM, this sort of communicational breakdown is necessarily impossible since the adherents share paradigms (ways of seeing the problem of interest which help to constitute the DM), symbolic generalizations (i.e., shared language and codes, where the same symbol or word means the same thing and is related to the same phenomenon for both people) and shared values, that is shared ideas about what is important; there may be degrees of differences but they are within the same framework. This does not imply lack of controversy within DMs; there can be vehement debates regarding theoretical issues, methods, interpretations of results, etc., but since the framework is shared, these controversies resolve. But communication between two such frameworks is very difficult though not impossible, since the same words might mean different things or refer to different

phenomena, or refer to the same phenomena in different ways. There is not necessarily any agreement about what is important and the individuals do not share important Gestalts through which they see the world. (For, after all, if they did, they would share a DM). Clearly, definitional fights can be resolved within a DM, but are almost irresolvable between two DMs as there is not the shared language, values or phenomena, nor is the interrelationship of language and phenomena the same.

With this sort of elaboration, it becomes clear that the differences between the two different DMs are more than a matter of differing paradigms, though that in itself can be rather confusing. Differing paradigms means that the same name may be used for different phenomena. Further, just to make matters more complicated, two different paradigms in all probability stand in different relation to their DMs, that is, no two paradigms are articulated in the same manner by their DMs. Keeping this difference in mind can help us to avoid certain mistakes which appear repeatedly in controversies.

For instance, in an attempt to present some of the difficulties in deciding among the various theoretical formulations of learning theory, Weimer and Palermo (1973, p. 230) state that,

...all these theorists utilized the 'empirical fact' of reinforcement in their behavioral equation, regardless of whether it was 'theoretically necessary' or not. As is by now common knowledge, the difficulty in deciding between the various theoretical formulations of 'learning theory' was that, despite their different appearances, all these theories 'predicted' (or, more often, postdicted) the same behavioral results. [their emphasis].

It might well be that the theories predicted the same results and that complicated the choice. But surely, when competing lines of research talk about the "empirical fact" of reinforcement, it is obvious that they are not necessarily talking about the same thing. Thus, as Weimer and Palermo (1973, p. 229) note:

As is well known, Hull related reinforcement directly to drive reduction and deemed it necessary for learning. Guthrie, by contrast, said that reinforcement was not necessary for learning (that learning was nothing but association by contiguity), and that it had an effect upon momentary performance only. Skinner was a strict reinforcement theorist (for operant behaviour) but considered it to be an experimental operation only, shunning all attempts to identify the nature of reinforcement with physiological (or other) variables. Tolman acknowledged the necessity of reinforcement for certain types of learning situations, but denied its relevance to others. Spence, --- remained noncommittal throughout the controversy ---.

Though all of the above learning theorists used the term "reinforcement" and referred to its presence as an empirical fact, it is clear they had different conceptualizations for the term, and actually, the identical term referred to different phenomena. However, in those DMs where reinforcement was regarded as essential to learning itself (rather than merely for performance), the reinforcement concept would be more important and would very probably be articulated and supported in a different manner than when reinforcement was regarded as useful only in eliciting the performance of the already learned response.

A related point is, that "further research" does not suffice to resolve controversies across DMs and in fact, cannot. Burgess (1972, p. 197) has also recognized this; he is speaking with reference to the

long-standing controversy between cognitive psychology versus Skinnerian psychology.

They are incorrect, however, when they note that research will be the final arbiter between these two approaches, this surely constitutes a misinterpretation of Kuhn -- nothing could be further from the truth. Research does not determine a paradigm change -- it is a Gestalt switch and since few Skinnerians or cognitists discuss their mutual problems this Gestalt switch seems far off.

Take for example, a controversy cited where a transposition experiment with two discriminable stimuli was agreed upon by both factions as an adequate instrument with which to test the alternative approaches. "The neobehaviouristic orthodoxy won this conflict by the simple, yet beautifully effective, expedient of 'operationalizing' the controversy in terms of their own experimental design," (Weimer and Palermo, 1973, pp. 230-231). By allowing the controversy to be couched in neobehaviouristic terms and methods, the cognitive proponents could not possibly "win." They had allowed the others to define the problem, conditions, methods of testing and acceptable solution, i.e., they had allowed the problem to be put into the neobehaviouristic DM, and, as such, had capitulated the fight before the fight. It is a bit like those expositions of the Socratic method wherein the "learner" answers questions so phrased that only one answer is possible, only to be drawn inevitably to the foregone conclusion. Allowing a debate to be entered only within one DM or another pre-empts the testing factor altogether.

In addition, the differences in values, meaning and language as well as choice of heuristic or metaphysical model, makes settling of terms

between two people who do not share a DM, exceedingly troublesome and improbable. This is not to say that communication and resolution is impossible; for example, areas of disagreement can be identified and discussed (although agreement is unlikely); however, discussion about the content of any of the intra-DM constituents by two people who do not share a DM is doubly difficult. First, they lack a shared framework of problems and meanings. Secondly, each constituent of a DM is not independent but rather is inter-dependent on the others; thus, the other constituents are often brought in to clarify a point about the constituent in question, but if they are, the entire DM and not merely one component, will be debated and that is fruitless. Here I think is one of the reasons Kuhn talks about conversion experiences.

In summary, the consensual aspects of the DM are of fundamental importance in facilitating normal science activities and ease of communication within a DM. Across DMs, however, this consensual aspect is lacking (by definition), and this deficit facilitates mis-understanding in a number of areas. First, semantic difficulties may arise because identical words have different meanings in different DMs. Secondly, different DMs may look at what appears to be the same phenomenon, but perceive very different problems and conceptualize different approaches; if these differences in perception are not recognized, mis-understandings are likely. Thirdly, the difficulty in understanding and communication is exacerbated by the fact that paradigms are not necessarily developed and elaborated in the same way by their respective DMs; thus, what one

DM may regard as the obvious next step and definitive trial may seem irrelevant, or even misguided to the other DM. Because of this, arguments about such activities as verification or disconfirmation, across DMs, are usually fruitless. Fourth, the inter-dependence of DM constituents often means that it is very difficult to fully discuss any one constituent without bringing in some others. Thus, it may be impractical to attempt to explain a DM's paradigm without also mentioning those values or models which help to articulate it; similarly, it would be very difficult to debate one's models without using the DM's symbolic generalization. Unfortunately, the debate then involves large regions of the respective DMs rather than isolated segments. Finally, misunderstandings across DM lines is fostered by the belief that these debates are resolvable by "further research". This belief is based on the premise that method and research are independent of DMs. This is clearly not the case. Perception and conceptualization of the phenomenon, language, models, instrumentation and methodology are DM-related. As such, further research rather than resolving the debate between two DMs, perpetuates and elaborates two parallel lines of research in disagreement. Communication and consensual understanding are DM functions for the adherents of each specific DM; this consensuality breaks down across DM boundaries, that is, consensuality is not a prerogative of those who do not share a framework.

Normal Science, Anomaly, Crisis, and Revolution

Normal science. Conceptualizing scientific activity and progress, Kuhn distinguishes between his own and Karl Popper's. Kuhn (1970a, pp. 242-243) argues that Popper's idea of 'revolutions in permanence':

...does not, anymore than 'square-circle', describe a phenomenon that could exist. Frameworks must be lived with and explored before they can be broken. But that does not imply that scientists ought not aim at the perpetual framework-breaking, however unobtainable that goal. "Revolution in permanence" could name an important and ideological imperative. If Sir Karl and I disagree at all about normal science, it is over this point. He and his group argue that the scientist should try at all times to be a critic and a proliferator of alternate theories. I urge the desirability of alternate strategy which reserves such behavior for special occasions.

Specifically, Kuhn conceptualizes scientific activity in terms of alternations between periods in which normal science is the dominant mode of activity, interspersed with revolutionary periods emerging when normal science has uncovered problems which are unsolvable in the usual conceptual-procedural structure. That is, normal science and revolutionary science alternate, but neither is dispensable in scientific activity.

Normal science consists of those research activities which articulate, elaborate and extend the paradigm or paradigm set, by engaging in puzzle-solving behavior. The paradigm is used in its various functions, is redefined and clarified, and the limits of its applicability began to be sensed. Essentially, lines of research originate from the paradigms and develop into DMs. Normal science activities investigate phenomena both deemed important by the value system of the developing DM and seen as

solvable by practitioners using the particular paradigm (and subsequent methodology and concepts built up by the DM thus far.) The paradigm is used in its Gestalt function and becomes progressively more "powerful" as a scientific tool.

During these periods of normal science, practitioners can take the current theory for granted, exploiting it rather than criticizing it; practitioners in the mature sciences are free to investigate their phenomena to "an esoteric depth and detail otherwise unimaginable" (Kuhn, 1970a, p. 247). It is at these times, that theories are investigated and extended, adding information to the body of knowledge already acquired; the journal article is the primary source of information, primary vehicle of information processing, and the mode of programs is cumulative, that is data build up and add to what has been previously articulated.

Kuhn points out that there is an aspect of normal science that is fundamentally conservative, in which normal science "seems an attempt to force nature into the preformed and relatively inflexible boxes that the paradigm provided" (1970b, p. 24). During normal science phases, there is really no sort of activity which is directed to eliciting new sorts of phenomena; new information about recognized phenomena, yes, but that is altogether different than revealing and having to deal with new phenomena. In fact, those phenomena that will not fit into the paradigm-DM complex are either not seen at all (Kuhn, 1970b, p. 24), or are "relegated" to other DMs, or to metaphysics. Despite this conservative aspect,

which makes possible investigation without constant glancing over one's shoulder, change does occur. At times in all substantive areas revolutionary science develops because of a rather specific developmental pattern. It is probably important to emphasize here that these revolutionary phases emerged from certain developments in normal science, that revolutionary science establishes the paradigms which are later articulated and which make normal science necessary and possible; suffice it to say that both forms of scientific activity are indispensable.

Anomaly. During the articulation and extension of the paradigms, that is during normal science activity, there sometimes occur obdurate discrepancies between paradigm-based prediction and actual empirical findings, i.e., anomalies. With replications, if these disparities persist, they result in a felt crisis, with resulting changes in the activities and goals of science, and the eventual establishment of a new paradigm or set of related paradigms, which initiates another period of normal science activity.

Kuhn (1970a, pp. 256-257) gives a rather nice example of this disparity between paradigm-induced expectation and empirical finding, in his summary of the origin of the Bohr atom.

...the background was an entirely normal puzzle. Bohr set out to improve the physical approximations...of the energy lost by charged particles passing through matter. In the process he made what was to him the surprising discovery that the Rutherford atom...was mechanically unstable and that a Planck-like ad hoc device for stabilizing it proved a promising explanation of the periodicities in Mendeleev's table, something else for which he had not been looking. At that point his model still had no excited states, nor was Bohr yet con-

cerned to apply it to atomic spectra; those steps followed, however, as he attempted to reconcile his model with the apparently incompatible one developed by J.W. Nicholson and, in the process, encountered Balmer's formula. Like much of the research that produces revolution, Bohr's biggest achievement in 1913 are products, therefore, of a research program directed to goals very different from those obtained ... and illustrates with particular clarity the revolutionary efficacy of normal research puzzles. (emphasis added)

Kuhn's explication aptly illustrates the manner in which anomalies crop up during the course of normal science activity, directed elsewhere, and how efficient or productive normal research puzzles prove to be for just this sort of thing. He makes this latter point several times, for instance, "...in developing sciences ... it is technical puzzles that provide the usual occasions and often the concrete material for revolution. Their availability together with the information and signals they provide account in large part for the special nature of scientific progress." (1970a, p. 247)

Since an anomaly is a discrepancy between paradigm-induced expectation and actual observation (Kuhn, 1970a, p. 52), it is important to realize, as Masterman points out, (1970, pp. 82-83) that:

[Kuhn's] essential point is that an anomaly is an untruth, or a should-be-solvable-but-is-insolvable problem, or a germane but unwelcome result, or a contradiction, or an absurdity, which is thrown up by the paradigm itself being pushed too far; not just an incidental counter-argument to the theory, or an awkward fact, which Kuhn correctly characterizes is merely an 'irritant'. Neither is it an extra-paradigmatic novelty nor a problem which used to exist in the field at an earlier stage, but which the developers of the paradigm have now suppressed and rendered invisible, because it is incompatible with the paradigms "basic commitment." The anomaly, to be a true anomaly, has got to be introduced from within

the paradigm.⁴ [her emphasis]

Efforts are made to "correct" the situation, at times by adjusting the analogy function of the paradigm, or alternatively, to produce variants of the theories which can accomodate the anomaly, or if worse comes to worse to "dig out the theory's fundamental assumptions, to try to make the analogy fit again" (Masterman, 1970, p. 83).

Obviously, anomalies will have different impact depending upon how important a point in a DM they challenge. An anomaly of theory for instance, may necessitate changes in a theory, without challenging the paradigm or other primary DM components. If the paradigm is challenged by the anomalies, however, then clearly they are very significant and will probably induce a relatively greater sense of difficulty.

Crisis. If the anomalies continue, if for instance there are several of them and they make the inadequacies of the present framework clear, or if the anomalies, though not numerous, are embarrassing because they counter critical parts of the DM, a period of crisis emerges. As Masterman develops the important point (1970, pp. 83-84)

...it is not only the case that a fully extended paradigm or theory, reaches the point where further extensions of it produce diminishing returns. The situation is worse.

⁴See Kuhn's example of obdurate anomalies in early twentieth century physics, 1970, p. 257.

The paradigm itself goes back on you, because you stretched it too far producing conceptual inconsistency, absurdity, misexpectation, disorder, complexity and confusion in exactly the same way as the crude analogy does, if pressed too far say, in a poem but quite unlike the way in which a system of pure mathematics does, when it yields undecidable formulae or contradictions, or fails to yield proofs, i.e., when an exact statement of what has gone wrong can still be made.

No philosopher of science before Kuhn had described this deterioration. All had blamed the gradual collapse of various scientific theories on the fact that they were eventually falsified in experience by, say, the emergence of new facts; i.e., on the non-cooperation, as it were, of nature. No one had blamed it on the fact that theories, since they have to have concrete analogical paradigms at the heart of them to define their basic commitments, and since the effect of these paradigms is drastically to restrict their fields, collapse, when extended too far, by their own make up...

This process, of emerging anomalies which increasingly demonstrate the inadequacies of the all important (to scientific activity) framework, where the paradigm "gets pushed too far" and collapses from within, have some of the tragicomic quality of the well-known cartoon character looking down and realizing he has run off the cliff and didn't know it. There's a ring of some of the same surprise, betrayal, incipient outrage, and dawning awful conviction that there's going to be hell to pay.

This sense of outraged and chagrined shock seems to me an earmark of that transition from normal science to revolutionary science. Awareness of the problem is necessary, for there is no crisis in Kuhn's framework without awareness. Crisis is induced by this awareness among adherents that anomalies exist and despite persistent efforts, will not conform to paradigmatic expectations (Kuhn, 1970b, pp. 67-68). Without such

awareness, there can be no crisis, though there may be anomalous findings.

Kuhn discusses some historical situations in which solutions to problems have been ignored for a time because no one knew they were needed; thus there was no crisis, but no new solution either, until awareness of anomaly induced crisis and the idea was dusted off to see if it would fit. The most famous of these situations is probably the anticipation of Copernicus by Aristarchus in the third century B.C. His work on the heliocentric solar system was ignored, as no one really needed it. It explained nothing then that the geocentric system did not, and there existed no felt crisis. Only after repeated failures of the Ptolemaic system did it become clear that an alternative was needed (summarized from Kuhn, 1970b, pp. 75-76).

A bit closer to home, Weimer and Palermo (1973, p. 232) present a similar situation. They regard K. Lashley's 1951 Hixton Symposium paper on serial order as

a coup de grace from which no behaviourism worthy of the name will ever recover...[but] it is only recently (nearly twenty years after the fact) that Lashley's monumental paper is receiving recognition. With behaviourism in sway, it was initially ignored even at Harvard (Lashley's own institution) which was subsequently the birthplace of the new psycholinguistics during the early 1960's.

(Lashley's paper was directly relevant to the psycholinguistic position).

Besides awareness, there are a number of other earmarks to the crisis phase, that is, the transition from normal science to revolutionary science. These include, "the proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent,

the recourse to philosophy and to debate over fundamentals ..." (Kuhn, 1962, p. 90, quoted by Masterman, 1970, p. 83, in a footnote). The proliferation of theories is an attempt to include the anomalies in the DM framework, with many of them more than a little plausible. Further characteristics of crisis are investigation of phenomena formerly considered beyond the confines of legitimacy, with a concomittant loosening of the rules of research, and debate about these rules of normal science.

Kuhn points out that it is not coincidental that the emergence of Newtonian physics in the 17th century, and relativity and quantum mechanics in the 20th, were preceded and accompanied by fundamental and philosophical investigations of the respective contemporary research traditions. It would seem that such periods of crisis must be interesting times in which to be a philosopher, and difficult times for most scientists.

Revolution. Crisis remains until someone formulates a paradigm that uses the anomalies as the base for his/her paradigm. Paradigms are often formulated, as Masterman points out (1970, p. 83) by "outsiders" who have a different viewpoint and a different set of techniques which they bring to bear on the anomalous phenomena and in the process of doing so, also create a paradigm and a new line of research. This does not mean that someone comes in, from another field, and incorporates the anomalies into his/her own original field; this is clearly not a revolution nor is the problem solution necessarily a paradigm; it may be just a problem solution. To be revolutionary, the new solution must serve as a paradigm

and begin a new line of research through normal science.

A new paradigm or paradigm set in revolutionary science demands that the conceptual and procedural commitments of the group fundamental to the practice of the relevant specialty be jettisoned and replaced (Kuhn, 1970a, p. 250); this often necessitates a redefinition of the relevant science (1970b, p. 102). Because the paradigm and subsequent DM are more than theory or model, some fundamental ways of seeing the world, and structuring phenomena, have changed.

Each revolution has consequences with ramifications for the past as well as the future.

The extraordinary episodes in which that shift in professional commitment occurs are...scientific revolutions. They are the tradition-shattering complements to the tradition-bound activity of normal science...

Each scientific revolution necessitated the community's rejection of one time-honored scientific theory in favor of another incompatible with it. Each produced a consequent shift in the problems available for scientific scrutiny and of the standards by which the profession determined what should count as an admissible problem or as a legitimate problem solution. Each transformed the scientific imagination in ways that we will ultimately need to describe as transformation of the world within which scientific work was done. Such changes, together with the controversies that almost always accompanied them, are the defining characteristics of scientific revolution (Kuhn, 1970b, p. 6).

It is necessary to emphasize that revolutions are part of a developmental sequence, a part that is non-cumulative, in which the older, established paradigm is replaced in whole or in part by an incompatible new one (Kuhn, 1970b, p. 91).

Some questions need to be raised at this junction. For instance,

what is revolutionary and what is just an extension of normal science? One aspect of this question was addressed above; if anomalies are absorbed by some other DM, a problem solution is not revolutionary. If the anomalies provide the base for a paradigm on their own, then revolutionary science is occurring. Kuhn approaches this question from a different angle (1970a, p. 251),

Can we distinguish mere articulations and extensions of shared belief from changes which involve reconstruction? The answer in extreme cases is obviously yes...Copernican astronomical theory was revolutionary but the caloric theory of adiabatic compression was not. These examples are, however, too extreme to be fully informative; there are too many differences between the people contrasted, and the revolutionary changes affected too many people. Fortunately, however, we are not restricted to them:...Lavoisier's discovery of oxygen...was revolutionary, for it was inseparable from a new theory of combustion and acidity. The discovery of neon, however, was not, for helium had supplied both the notion of an inert gas and the needed column of the periodic table.

Another relevant question might be: Revolutionary for whom? Again, Kuhn (1970a, p. 242),

Sometimes the answer is easy: Copernical astronomy was a revolution for everyone; oxygen was a revolution for chemists but not for, say, mathematical astronomers...For the latter group, oxygen was simply another gas, and its discovery was merely an increment to their knowledge; nothing essential to them as astronomers had to be changed in the discovery's assimilation. It is not, however, usually possible to identify groups who share cognitive commitments simply by naming a scientific subject matter--astronomy, chemistry, mathematics, or the like. Some scientific subjects, for example, the study of heat have belonged to several different scientific communities at different times, sometimes to several at once without becoming the special province of any.

It should be noted here that "crisis" does not always refer to the

discipline as a whole. This partially depends upon the significance of the anomalies--the greater their significance, the greater the probability that a large group of scientists is affected. It is important to recognize that the relevant group here is that group of practitioners constituting and working in the DM that the anomalies are challenging. If a discipline is comprised of several DMs, one or perhaps more may be experiencing crisis, but not necessarily all of them.

Similarly, as Kuhn alluded above, it is necessary to know the nature and structure of group commitments before being able to signify those to whom a paradigm is a revolution. It would seem that those who responded vociferously to the paradigm, either positively or negatively, would be the scientists most involved in the fate and consequences of the issue, (though unfortunately, this criteria would fail to pick up those practitioners who, maintaining a stoney silence, hope the offending party will go away if ignored long enough). At times there is an easily identified community, while for other phenomena, interested scientists are dispersed over a variety of disciplines, and meet primarily at specialized conferences.

Kuhn very clearly feels that the "unit of analysis" with regard to revolutions are DMs, that is,

...the practitioners of a given specialty, men [sic] bound together by common elements in their education and apprenticeship, aware of each other's work, and characterized by the relative fullness their professional communication and relative unanimity of their professional judgment. In the mature sciences the members of such communities would ordinarily see themselves and be seen by others as the men exclusively responsible for a given subject matter

and a given set of goals, including the training of their successors. Research would, however, disclose the existence of rival schools as well; typical communities at least on the contemporary scientific scene, may consist of a hundred members, sometimes significantly fewer. Individuals, particularly the ablest, may belong to several such groups, either simultaneously or in succession and they will change or at least adjust their thinking caps as they go from one to another.

Groups like these should, I suggest, be regarded as the units which produce scientific knowledge...

This is also the body which decides the fate of the new paradigm, and articulates it into a line of research if it is accepted.

Changing paradigms also directly implies changing from an established DM to a developing DM, with all this involves in changes of perception, meaning, and relevant phenomena. In changing DMs, a scientist, while usually doing so on intellectual grounds, is also by implication deciding between systems which heavily influence his/her perception and experience, as well as theory-building. It is not surprising that new DMs are often resisted; depending upon the scientist's present DM, new DMs must seem more or less congenial. The congeniality of developing DMs would vary widely, depending upon how closely the scientist's present DM approached the various emerging DMs in their influence on perception and experience of the world, as well as for purely logical considerations in theory-building and such.

Discussing these problems regarding paradigm choice, Kuhn describes the issues as:

...about techniques of persuasion or about the argument or counter-argument in a situation in which...neither proof nor error is at issue. The transfer of allegiance from

paradigm to paradigm is a conversion experience that cannot be forced. Life-long resistance...is not a violation of scientific standards but an index to the nature of scientific research itself...Though the historian can always find men--Priestley, for instance--who were unreasonable to resist for as long as they did, he will not find a point at which resistance becomes illogical or unscientific (1970a, p. 260).

Precisely because allegiance is in part illogical, the continued resistance to change is not, in an illogical context, an illogical process. Continued resistance to change, or decision to make the change, cannot be judged, nor decided, using purely logical criteria because, commitment to a DM means scientific research itself is not always logical to a DM.

Kuhn points out that in the debate about DM choice,

...neither party has access to an argument which resembles a proof in logic or formal mathematics. In the latter, both premises and rules of inference are stipulated in advance. If there is disagreement about conclusions, the parties to the debate can retrace their steps one by one, checking each against prior stipulation. At the end of that process, one or the other must concede that at an isolable point in the argument he has made a mistake, violated or misapplied a previously accepted rule. After that concession he has no recourse, and his opponents' proof is then compelling. Only if the two discover that they differ about the meaning or applicability of a stipulated rule, that their prior agreement does not provide a sufficient basis for proof, does the ensuing debate resemble what inevitably occurs in science (1970a, pp. 260-261).

This state of affairs is directly attributable to two factors: the fact that decision and commitment to paradigm and DM involve other processes than the purely logical, and secondly, that these not-logical processes are more fundamental than the explicitly articulated theory. Since the

level of debate is equally restricted to this latter theoretical level, the non-logical material continues its influence and the debate continues, without resolution and at the wrong level.

However, people do in fact make paradigm decisions, based in part on these not-logical processes. Kuhn feels these choices are based on reasons which include "accuracy, scope, simplicity, fruitfulness, and the like" (1970b, pp. 261-262). He goes on to emphasize the importance of scientists being taught to value these characteristics and being provided with examples that illustrate them in actual use; if scientists did not hold such values for guidance obviously their disciplines, and scientific activity, would develop very differently. So Kuhn is saying that values constitute the differential criteria for paradigm choice.

I am...insisting that such reasons constitute values to be used in making choices rather than rules of choice. Scientists who share them may nevertheless make different choices in the same concrete situations. Two factors are deeply involved. First, in many concrete situations, different values, though all constitutive of good reasons, dictate different conclusions, different choices. In such cases of value-conflict (e.g., one theory is simpler but the other is more accurate) the relative weight placed on different values by different individuals can play a decisive role in individual choice. More important, though scientists share these values and must continue to do so if science is to survive, they do not all apply them in the same way. Simplicity, scope, fruitfulness, and even accuracy can be judged quite differently (which is not to say that they may be judged arbitrarily) by different people. Again, they may differ in their conclusions without violating any accepted rule (1970a, p. 262; emphasis added)

Kuhn further specifies that once a DM is in existence, it is the prerogative and the responsibility of that community of scientists to formu-

late scientific values for their DM and then to both apply and accept responsibility for those value applications.

Problems with Using the Kuhnian Schema

Textual difficulties. Kuhn's 1970 revision is primarily directed toward differentiation of his original paradigm notion into paradigm and DM. Unfortunately, he presented this revision in a postscript to his 1970 edition of The Structure of Scientific Revolutions, where it was often overlooked, and where he allowed it to exist in isolation from the rest of the volume; that is, Kuhn did not revise his schema in light of the paradigm-DM differentiation. His 1970 edition is essentially his 1962 schema, and a postscript containing the DM revision, with no reworking of the text of the book.

This has had an unfortunate effect, in that most who used Kuhn's schema either did not or could not integrate the revision into their work and thus have perpetuated and elaborated the ambiguities, e.g., the comments by Burgess (1972, p. 193) regarding polemic in psychology (already cited), or Palermo (1971, pp. 136 & 138). Similarly, see M. Brewster Smith (1973a, p. 464) in Elms (1975, p. 967), "Our best scientists are floundering in the search for a visible paradigm." These, among others, have encountered difficulties because Kuhn, for whatever reason, did not adequately revise his 1970 publication. It is clear, however, that enough confusion and ambiguity has been engendered in psychology for Lipsey (1974, p. 406) to decry the use of Kuhn as a template.

Even careful authors have run into difficulties which appear to originate not because the revision was unknown to them, but rather because its ramifications were in no way articulated by Kuhn. For instance, Warren (1971, p. 407) and Palermo (1971) both use earlier versions of the schema, while it appears they knew of the revision. Weimer and Palermo (1973, p. 273, footnote B), divide the Masterman-Kuhn paradigm usages into "exemplar" and "sociological," the latter of which can be divided into "theoretical" and "metaphysical". It is difficult to understand where they arrived at these "exemplar" and "sociological" categories, as both Masterman and Kuhn clearly use "artefact" (roughly analogous to exemplar) "sociological" and "metaphysical". Similarly Warren (1974, p. 409) mentions "sub-paradigms." What are they? Watson notices the ambiguities in the paradigm concept (1974, p. 52), but ambiguities in his presentation remain because of the paradigm-DM split not being articulated. And it should be emphasized, the above-named authors have been among the careful and their problems with Kuhn are relatively minor. With others, the problems are considerable. Palermo (1971, p. 138) argues that psychology has had two paradigms (he apparently means DMs). Lipsey (1974, p. 407) argues that psychology is mis-paradigmatic, rather than pre- or post-paradigmatic; though he, again, is semantically inaccurate, his point will receive corroboration in later chapters.

Conceptual difficulties.

Power of paradigm concept. A number of conceptual difficulties

remain. D. Shapere raises a series of criticisms about Kuhn's DM revision, some of which are relevant. Shapere examines the implications of Kuhn's 1970 revisions for the latter's original framework.⁵ First, he finds the paradigm-DM differentiation of little help for those who originally found Kuhn's paradigm concept obscure, as neither version clarified how the remaining factors covered by the paradigm term were related to or embodied in the historical examples in such a way that the whole outlook of the tradition could be conveyed. In other words, what were the routes and processes by which concrete paradigms were linked to scientific traditions? It would seem that Shapere's criticism is answered in part by the DM concept itself, in which paradigm is articulated into a more encompassing and complicated network of method, commitment and theory.

Shapere contends that Kuhn has not yet clarified the ways in which the physical science's "general" or "over-arching" paradigm determines the course of scientific research and judgment. Shapere perceived Kuhn's paradigm concept to be

a single overarching Weltanschauung, a disciplinary Zeitgeist, that determined the way scientists of a given tradition viewed and dealt with the world, that would determine what they would consider to be a legitimate problem, a piece of evidence, a good reason, an acceptable solution and so on." (1972, p. 707).

⁵ Shapere (1972, p. 706) makes several other charges, most notably about paradigm's ambiguity and relativism. Masterman had more than adequately dealt with the first and Shapere's charges regarding relativism lose a good deal of their efficacy because he refuses to regard the role of values in science.

Shapere notes (1972, p. 707) that it was "precisely the unity, and the controlling status of paradigms that constituted the appeal and the challenge of Kuhn's original view". It is Shapere's opinion that, if Kuhn abandons the controlling status of the paradigm, he will have abandoned what was one of the most provocative and influential aspects of his earlier view.

Shapere has raised an interesting point. Kuhn's original concept of paradigm did indeed have the type of controlling status to which Shapere refers. By revision to a concrete problem solution with analogue functions, the paradigm concept has been removed from the more general, implicit, and thus influential role, leaving a conceptual vacuum for precisely this over-arching concept already described. In examining the family work area, interestingly enough, there are indications of the need for such a concept at this broader, general level. Such a concept would label several processes and characteristics of what appear to be two major groups of family therapists that differ in their commitments to exactly something like a *Weltanschauung*. (These two groups and identification of their points of difference, as well as some implications for Kuhn's schema, will be reviewed at length in Chapter VIII).

Another of Shapere's criticisms, which seems to me related, concerns Kuhn's short treatment of the DM. He in fact spends a good deal of that cursory treatment distinguishing among the four DM components he identifies, without discussing any unity underlying them (1972, p. 707). While I would take issue with Shapere and point out that the paradigm set con-

stitutes a firm source of unity, it is also clear that Shapere's point is well taken. Kuhn's DM concept is neither set into context, described in terms of unifying themes or processes, nor related very well to such things as theory or instrumentation (though some attempt to start the latter was attempted here in Chapter II). Consideration of these issues will most likely benefit Kuhn's schema.

"Inclusive perimeter" of the DMs. Another set of conceptual difficulties relates to what can be termed the "inclusive perimeter" of a DM. How "big" is a DM; that is, what is the size of a DM and what are the criteria that determine this? Is the "size" determined by number of adherents or breadth of the paradigm application? Kuhn is quite ambiguous in this regard. He states that communities of practitioners

...exist, of course at numerous levels. The most global is the community of all natural scientists. At an only slightly lower level, the main scientific professional groups are communities: physicists, chemists, astronomers, zoologists, and the like. For these major groupings community membership is readily established except at the fringes... Similar techniques will also isolate subgroups: organic chemists, and perhaps protein chemists among them, solid state and high energy physicists, radio astronomers, and so on. It is only at the next lower level that empirical problems emerge. How, to take a contemporary example, would one have isolated the phase group, prior to its public acclaims? For their purpose, one must have recourse to attendance at special conferences, to the distribution of draft manuscripts or galley proofs prior to publication, and above all, to formal and informal communication networks including those discovered in correspondence and the linkages among citations... Typically, it may yield communities of perhaps one hundred members, occasionally significantly fewer (Kuhn, 1970b, pp. 177-178).

The first difficulty is in trying to identify the extent of what will be called the "inclusive perimeter." Which of these communities

is co-extensive with the term DM? If the DM governs a group of practitioners and not the subject matter, what level of group, then, does it govern? Does Kuhn's DM concept include the system of all-natural scientists-and-their-network-of commitments, or is it rather at the levels of all-biologists-with-their-commitments, or all vertebrate biologists, etc.? Are all of these DMs and is the term DM infinitely extensible to all levels and if so, what are the consequences? Should the use of the term "DM" be reserved, rather, for some relatively specific level of community and a new term be found for something like all-natural-scientists?

There are several considerations in support of this latter suggestion. If the paradigm is the concrete problem solution, then its articulation in the form of a DM would also be fairly specific. For example, from earlier in the chapter, Galileo's solution of the ball rolling down the inclined plane helped him to solve the pendulum problem (by considering the bob as a point-mass); Huyghens then used Galileo's solution on the problem of the center of oscillation and Bernoulli in turn used Huyghen's solution for the flow of water from an orifice. The paradigmatic use of the original solution remains very specific, grounded in concrete problems. It is usually possible to name the practitioners who are active in lines of research, and it appears that it is usually a relatively small group of practitioners who share paradigms and other DM constituents. Henceforth, the inclusive perimeter of DMs as referred to here, will be the standard in later chapters during discussions of the

family therapy DMs. Conclusions can be made later whether such usage was helpful or not.

Use with the social sciences. Of greater significance is the problem that the Kuhnian framework was developed for the natural sciences, not the social sciences. In fact it was Kuhn's observation that in contrast to natural scientists the controversies among social scientists regarding fundamentals led him to develop the paradigm concept (Kuhn, 1970b, p. viii). And actually, when one looks at what subject matter he addresses himself to, Kuhn has remained within the mature physical sciences -- astronomy, physics, chemistry, electricity, and though only occasionally, geology (he mentions Lyell).

Others have also noticed Kuhn's focus on the established sciences. Palermo (1971, p. 138) notes that in fact Kuhn appears unsure of the status of the social sciences in general, at times implying that they have not yet reached the maturity of paradigmatic-status. Warren (1971, p. 408) concurs with Palermo and points out that it was the contrast "between the social processes of the physical and the behavioral sciences" that provided the impetus to Kuhn's theorizing.

This proved to be an important point, as it is becoming painfully obvious that the social sciences in general, and psychology specifically, does not have the same basic consensus that the mature physical sciences do regarding fundamentals of value, problem choice, and appropriate approach. The extensively documented felt crisis and the issues contained therein testify to that, as do certain authors who, in relation

to a Kuhnian analysis, have directed attention to this. Watson (1974, p. 52) puts it in terms of psychology lacking "this universal agreement about the nature of our contentual paradigm [sic]." He points out that there remains debate over fundamentals and in research, "findings stir little argument but the overall framework is still very much contested." Burgess (1972, p. 196) is of the same opinion.

Fortunately, in taking Kuhn to task for his confused view of preparadigm science, Masterman addresses the issue of what she terms "multiple-paradigm" science. She states that he fails to distinguish three states: "non-paradigm science, multiple-paradigm science, and dual-paradigm science. Non-paradigm science is the state of affairs right at the beginning of the process of thinking about any aspect of the world..." (1970, p. 73).

Masterman (1970, p. 75) simplifies the position, by saying squarely that when "normal science sets in," anywhere there you have science, and where it does not set in, there you have philosophy, or something else, not science and that it is always some construct using, puzzle-solving trick which starts off normal science" [her emphasis]. She considers the mature physical sciences as having one "vast paradigm" and to have created through the convergence of a number of paradigm-guided research lines, i.e., DMs, which mutually threw light on one another (1970, p. 75). This can be seen in juxtaposition

...with multiple-paradigm science, with that state of affairs which, far from there being no paradigm, there are on the contrary, too many. (This is the present overall situation in the psychological, social, and information sciences.) Here, within the subfield defined by each paradigmatic technique,

technology can sometimes be quite advanced, and normal research puzzle-solving can progress. But each sub-field as defined by its technique is so obviously more trivial and narrow than the field as defined by intuition, and also the various operational definitions given by the techniques are so grossly discordant with one another, that discussion on fundamentals remains, and long run progress (as opposed to local progress) fails to occur. This state of affairs is brought to an end when someone invents a deeper, though cruder (p. 23) paradigm, which gives a more central insight into the nature of the field making research into it more rigid, esoteric, and precise (p. 18 and 37). This (p. 16) either by causing rival, more shallow paradigms to collapse, or alternatively, by attaching them somehow or other to itself, triumphs over the rest, so advanced scientific work can set in, with only one total paradigm. Thus, multiple-paradigm science is full science, on Kuhn's own criteria; with the proviso that these criteria have to be applied by treating each sub-field as a separate field (p. 74).

Masterman is of the opinion that multiple-paradigm science is full science and may be treated to a Kuhnian analysis by treating each sub-field as a separate field. Burgess (1972, p. 196) concurs with Masterman that psychology is multiple-paradigmatic; for instance,

What paradigms (in the sense of multiple-paradigms noted above) exist in psychology? Lichtenstein; adopting a Kuhnian model mentions a number (1971): Fechner's psychophysics; Titchener's structuralism; Pavlov's conditioned response; Hull's hypothetico-deductive model; Köhler's isomorphism and finally Skinner's operant (1971, p. 6). Krasner (1971) has a different view, he believes behaviour therapy constitutes a paradigm. Katahn and Koplin (1968) believe that the clash between Breger and McGaugh (1965) on the one hand and Weist (1967) on the other represents a paradigm clash, while Keehn (1969) believes the Skinner's 3-term contingency ($SD-R-S^R$) constitutes a paradigm. Clearly, there are differences here. The models used by Lichtenstein, Katahn and Koplin are far more general than that used by Krasner. What can we say of the choice made? Clearly, it was not Hull's hypothetico-deductive model that was attacked, it was his drive-reduction hypothesis as well as his strong interpretation of or reliance on reinforcement. His clashes with Tolman and his followers (why does Tolman's interpretation not qualify as a paradigm?) did not centre around

Hull's use of the hypothetico-deductive method, they centred around the role of reinforcement (hence all the studies on latent learning) and on drive-reduction (on drive induction and explanatory behavior etc., etc.-vide Bolles (1967)).

Burgess believes that Masterman's "analysis of paradigm has essentially saved this concept for psychology -- especially her three-fold historical division of scientific development" (1972, p. 196); I would concur with Burgess' opinions. Warren (1971, p. 411, footnote) apparently agrees, and uses the revision appropriately.

The analysis regarding normal science and multiple-paradigm disciplines is quite appropriate for psychology and theoretically at least allows for a Kuhnian analysis, as long as the focus is on a sub-field. This is obviously another reason to have limited the focus of Kuhnian analysis to one of the family therapy "schools"; as the analysis continues, it will become more clear that the particular "school" constitutes a Kuhnian DM, with its own paradigm, DM components and developments.

C H A P T E R I I I

ANOMALIES AND THE ATTEMPT AT ACCOMODATION IN PSYCHOANALYSIS

The purpose of this section is to demonstrate that the emergence and development of the D-B family therapy "school" can be productively analyzed in Kuhnian terms. Emergence of the DB DM was a response to anomalies which developed as traditional psychoanalytic theory and practice and were extended to new clinical phenomena. For these reasons, the inception of the clinical psychoanalytic DM will be sketched to provide a conceptual starting point and a contrast against which the emergence of these psychoanalytic anomalies can be seen. Though psychoanalysis was the dominant and virtually undisputed, clinical approach for a half-century (a situation, many would argue, which continues into the present), only the early elaborations of the classical psychoanalytic paradigm, and then the relevant anomalies will be addressed; the task of tracing the comprehensive psychoanalytic elaboration has not been the intention of this author, and is beyond the scope of the present work.¹ A Kuhnian analysis of the entire psychoanalytic

¹It is also necessary to be circumspect in speaking of "the psychoanalytic DM" as at present, it appears that there is none, or rather, that there are several. For the purpose of this dissertation, I will be using the very early Freudian paradigm (henceforth referred to as the classical or interpretationist paradigm, and DM) which Freud developed regarded as bounded in time, and summarized, by Freud's 1914 paper on "Recollection, Repetition, and Working Through"; after this phase, both technique and conceptualization changed. The anomalies relevant to the emergence of family therapy, however, occurred with respect to Freud's classical paradigm and DM, so I will review only its inception and early development. It should be noted here that the

framework will not be a focus here, in part because it is not strictly relevant, and in part to avoid the infinite regression of "DB from psychoanalytic anomalies; psychoanalysis from neurology's anomalies, neurology from..."

Freud's Revolutionary Paradigm

Problem of hysteria. The psychoanalytic DM developed around the problem of hysteria. During the middle and late 19th century, hysteria was the province of neurology, spurred on by its resounding success in having found a biological etiology for general paresis. Neurology was concentrating its efforts on biological explanations and the attacks of hysteria with very little success. By the late 19th century, hysteria as a disorder constituted an important medical problem. In Kuhnian terms, it also constituted an anomaly for neurology. Also in Kuhnian terms, the problem Freud faced was the treatment or "cure" of hysteria. At the relevant point in Freud's life, when he was a practicing clinician (in neurology) doing private work, the problem to which he was primarily

original classical paradigm continued to be elaborated by one group, the interpretationists, yet was also modified by other groups into a number of other DMs which developed in parallel to the classical or interpretationists position. Thus the work of the ego analysts constitutes a separate DM as does the work of Melanie Klein. These two DMs were clearly developments in psychoanalysts thought and practice, but they differed sufficiently from the classical DM with respect to paradigms, symbolic generalizations, models and values, to constitute separate DMs in their own right. As the anomalies relevant to family therapy's emergence are not directly relevant to these later parallel psychoanalytic DMs, these DMs will be acknowledged, but not explicated.

addressing himself was the cure of hysteria, and explanation or technical innovation were in the service of such a cure. This cure would constitute the solution to the problem, and if used analogically (which it was for other neuroses), the paradigm.

At that time, a "cure" for hysteria was quite important for Freud. Upon his return in 1886 from studying with Charcot in Paris, Freud specialized in nervous diseases, with hysteria providing the clinical picture in a large proportion of his patients. Initially he relied upon the current methods of treatment, including,

hydrotherapy, electro-therapy massage, and the Weir-Mitchell rest-cure. But when these proved unsatisfactory his thoughts turned elsewhere. 'During the last few weeks,' he writes to his friend Fliess on December 28, 1887, 'I have taken up hypnosis and have had all sorts of small remarkable successes.' (Strachey, 1955, p. xi).

Apparently, from his own subsequent account, Freud began to use hypnosis to achieve recollection and later also catharsis in the patient rather than for its standard use as a suggestion technique (Strachey, 1955, p. XI).² Freud first began to use the new method sometime between 1886 and 1893 (Strachey, 1955, p. XII).

First technical innovation: role of recollection. Freud's first technical innovation, by approximately 1887, was the use of hypnosis for

² Although there are a number of definitions and conceptualizations of "catharsis", there is agreement that catharsis is a process wherein an individual emotionally experiences or re-experiences something, and to some extent releases or purges pent-up affect or emotion.

recollection or recall, which was conceptualized as the operative process of change. While hypnotized, the patient would be induced to recollect repressed material and with that recollection, the repressed was remembered and symptoms were no longer "needed" to express the material. Freud's later account of this appears quite matter-of-fact considering the reception it gained him from colleagues.

Now in those days of hypnotic treatment 'recollection' took a very simple form. The patient put himself back into an earlier situation, which he seemed never to confound with the present, gave an account of the mental processes belonging to it, in so far as they were normal, and appended to this whatever conclusions arose from making conscious what had before been unconscious. (S. Freud, 1914a, p. 367).

Breuer and Freud later supplied the rationale for the mechanism of change. In 1892, in their "...Theory of hysterical attacks," they stated:

The memory which forms the content of the hysterical attack is an unconscious one, or, more correctly, it is part of the second state of conscious which is present in more or less highly organized shape in every hysteria. Accordingly, that memory is either wholly absent from the patient's recollection when he is in his normal state, or is present only in a summary way. If we can succeed in bringing such a memory entirely into normal consciousness, it ceases to be capable of producing attacks...the psychical events during his attack remain hidden from him. They can, however, be awakened at any time by hypnosis. (Breuer and Freud, 1892, Vol. V, p. 29).

The operative process was recollection, which allowed the isolated memory to be associated with other cognitive material, in a sort of "network" and gradually lose its power and isolation through repeated references and associative ties. Emphasis was directed toward those events which excited symptom-formulation. "Reproduction of the mental

processes" evoked in the particular situation were thought to "bring about a release of them through conscious operations." (Freud, 1914a, p. 368)

Second technical innovation: the role of affect. By 1893, the role of affect had become more clear. In their "Preliminary Communication", Breuer and Freud noted:

we found, to our great surprise at first, that each individual hysterical symptom immediately and permanently disappeared when we had succeeded in bringing clearly to light the memory of the event by which it was provoked and in arousing its accompanying affect, and when the patient had described that event in the greatest possible detail and had put the affect into words. Recollection without affect almost invariably produces no result. (1893, Vol. II, p. 6)

The operative process of cure, or mechanism of change of catharsis was now added to the original recollection formulation.

It brings to an end the operative force of the idea which was not abreacted in the first instance, by allowing its strangulated affect to find a way out through speech; and it subjects it to associative correction by introducing it into normal consciousness (under light hypnosis) or by removing it through the physician's suggestion, as is done in somnambulism accompanied by amnesia. (Breuer and Freud, 1893, Vol. II, p. 17)

Freud's later, stable definition of abreaction held that "quantities of affect pent up by repression...(1914a; p. 376) are released, identified, and articulated.

Third technical innovation: free association and repetition. Though hypnosis allowed him all this, Freud realized that he was not particularly successful at its induction, and gradually gave it up, contenting him-

self with putting patients into a state of "concentration", using occasionally a gentle pressure on the forehead. (This was the case by approximately 1895 as he mentions this in Studies on Hysteria (pp. 107-108) with an engaging mixture of chagrin and pride).

With the virtual abandonment of hypnosis and the development of free association, Freud's technical innovations reached the third phase; the continuation of technical innovation and exploratory concepts provided him with a stable and effective method for "curing" hysteria.

It was Strachey's (1955, Vol. II, p. XVII) opinion that this shift from hypnosis to free association revealed the presence of "resistance" and this essentially constituted a critical choice point for Freud.

How was this unwillingness to be dealt with? Was it to be shouted down or suggested away? Or was it, like other mental phenomena, simply to be investigated? Freud's choice of this second path led him directly into the uncharted world which he was to spend his whole life exploring.

Having made his decision to explore rather than subdue resistance, Freud concentrated on two phenomena which promised to be helpful: dreams and the resistances themselves (as phenomena rather than obstacles).³ Dream analysis provided both a method with which to investigate primary

³ Also, the death of his father prompted Freud to begin analyzing his own dreams; a good deal of this work went into Freud's 1897 Interpretation of Dreams.

process, and also the influence of primary process on conscious thought (i.e., the connections between primary and secondary process). Strachey (1955, Vol. II, p. XVIII) has pointed out that this in turn, put Freud "... in possession of a new technical device - interpretation." (The subsequent importance of the function of interpretation will be reviewed). Dream analysis allowed Freud to carry out his own analysis, to formulate the Oedipus complex and infantile sexuality, and to recognize, albeit dimly, the obstacles and therapeutic potentials of "transference". (Strachey, 1955, Vol. II, p. XVIII).

The emergence of the importance of interpretation as a technique and transference as a phenomenon, were underscored by their similar centrality when Freud attempted to investigate resistances as phenomena. Because of the importance of resistances and relatively little control over the patient's locutions during free association, Freud was forced to deal with the patient's compulsion to repeat; he eventually set out to induce repetitions as a method of dealing with the resistances. Freud describes the compulsion to repeat as a process wherein, rather than remembering what has been repressed, the patient

expressed it in action. He reproduces it not in his memory but in his behavior; he repeats it, without of course knowing that he is repeating it.

For instance, the patient does not say that he remembers how defiant and critical he used to be in regard to the authority of his parents, but he behaves in that way toward the physician. (Freud, 1914a, p. 369).

By inducing these repetitions, the hitherto unconscious conflict became

manifest to the analyst and also became "present" in actual life, and therefore accessible to intervention. The repetitions were induced by heightening the transference of the patient; for example, by remaining relatively silent, or giving very little personal reality based information, and by not allowing the patient to see facial expression.

In contrast to hypnosis, the repetition compulsion was viewed as a "piece of real life" (1914a, p. 371) and therefore not always an indifferent or trivial phenomena. At times, this could result in an exacerbation of symptoms during treatment (1914a, p. 371); this point became particularly cogent and important later for patients characterized as latent schizophrenic or borderline (one class of later anomalies), where exacerbation often provoked frank psychoses. Also, it became clear that the repetitions were evident not only in the transference, but also in all other aspects of the person's life (1914a, p. 370).

We must be prepared to find, therefore, that the patient [repeats] also in all other matters occupying and interesting him at the time, for instance, when he falls in love or sets about any project during treatment. (Freud, 1914a, p. 370).

This was the reason for invoking the psychoanalytic injunction against major life changes during analysis. Since the resistances were eventually regarded as determining the succession of repetitions (p. 371) and also, since the greater the resistance, the more extensively the repetition compulsion replaced the capacity or willingness to insight (or recollection), (p. 370), resistances became the focus of attention; once elicited, the analyst used interpretation to reveal the relation

between resistances during free association, and unconscious material, allowing the patient to "work through" the resistances. In this third phase,

the analyst abandons concentration on any particular element or problem, contents himself with studying whatever is occupying the patient's mind at the moment, and employs the art of interpretation mainly for the purpose of recognizing the resistances which come up in regard to this material and making the patient aware of them (1914a, p. 367).

It would appear that recollection is once again the aim of analysis; but Freud is emphatically clear that this is not the case. He stresses that the repetition compulsion and its shielding resistance as expressed in the transference must be "worked through." The first step in overcoming the resistance is

made...by the analyst's discovering the resistance, which is never recognized by the patient, and acquainting him with it. Now it seems that beginners in analytic practice are inclined to look upon this as the end of the work.. naming the resistance could not result in its immediate suspension. One must allow the patient time to get to know this resistance of which he is ignorant, to 'work through' it, to overcome it, by continuing the work according to the analytic rule in defiance of it. Only when it has come to its height can one, with the patient's cooperation, discover the repressed instinctual trends which are feeding the resistance; and only by living them through in this way will the patient be convinced of their existence and their power. (1914a, p. 375).

The working through of the resistances as they arise in the transference was regarded by Freud as the process that affected the greatest changes in the patient and that could be correlated theoretically, with the change function of abreaction in the earliest formulation.

(1914a, p. 376). "Working through" involved emotional elements, and recollections once the resistances had been overcome (p. 375). In all his formulations of cure, Freud included a recollection component, and an emotional "reliving" of some sort. In this third phase, the role of interpretation assumes technical importance because of its function with regard to the shielding resistances; interpretation became the technique which induced the working through of resistances, by which the therapeutic emotional and insightful aspects were elicited.

The paradigm and its revolutionary aspects. With the development of free association and the change mechanism of working through, Freud re-conceptualized hysteria and his problem solution constituted a revolutionary paradigm.

It should be emphasized that the series of technical innovations (hypnosis for recollection, then for recollection with catharsis, then free association and interpretation) preceded any re-conceptualization of a change mechanism or of the disorder of hysteria. It is these technical innovations and supporting conceptualizations which constitute the classical Freudian or psychoanalytic paradigm.

The technical innovations are supported by their respective mechanisms of change, and then by the re-conceptualization of hysteria as a psychological rather than biological disorder. Strictly speaking, the third-phase innovations constitute the paradigm; the techniques and change processes of the first two phases were successful in a limited way in comparison to the innovations of free association and

interpretations. Also, either of the first two phases would have difficulty as a theory-practice system if used as an analogue and extended to other neuroses. First, theoretically because they were too circumscribed and secondly, empirically, because hypnosis is rarely successful with obsessional or paranoid individuals. Finally, for Freud to develop this series so rapidly, despite the successes of the first two phases, one can infer that he felt, or recognized, some inadequacies in the earlier phases that induced - allowed him to develop the third, then stay with it.

The technical innovations of free association which elicits the "raw data" of psychoanalysis, and of interpretation, which supplies the tool to deal with such data, constitute the relevant paradigm techniques. Freud's postulation that the mechanism of change was repeatedly working through both the cognitive and emotional aspects in the transference provided the conceptualization of cure which accompanied the new techniques. Similarly, both the techniques and change mechanisms were useful in the amelioration or cure of other types of neuroses, i.e., obsessional and phobic. Thus, Freud's problem-solution admirably filled its paradigmatic "analogue" function for new phenomena.

Finally, the innovations induced a re-conceptualization of the hysteria, in stages. First, Freud and Brewer wrote in 1893 that the symptoms of hysteria were meaningful and not random as hitherto conceptualized.

Let us keep firmly in mind the fact that it is the distressing antithetic ideas (inhibited and rejected by normal consciousness) which press forward at the moment of emergence of the disposition to hysteria and find their way to the somatic innervation, and we shall then hold the key to an understanding of the peculiarity of the deliria of hysterical attacks as well. It is owing to no chance coincidence that the hysterical deliria of nuns during the epidemics of the Middle Ages took the form of violent blasphemies and unbridled erotic language or that (as Charcot remarked in the first volume of his Leçons du Mardi) it is precisely well-brought-up and well-behaved boys who suffer from hysterical attacks in which they give free play to every kind of rowdiness, every kind of wild escapade and bad conduct. It is the suppressed--the laboriously suppressed--groups of ideas that are brought into action in these cases, by the operation of a sort of counter-will, when the subject has fallen a victim to hysterical exhaustion. Perhaps, indeed, the connection may be a more intimate one, for the hysterical condition may perhaps be produced by the laborious suppression; but in the present paper I have not been considering the psychological features of that condition. Here I am merely concerned with explaining why -- assuming the presence of the disposition to hysteria -- the symptoms take the particular form in which we in fact observe them. (1893, p. 53).

The contention that hysteria was of psychological etiology came only later in 1895 (Strachey, 1955, p. XXIV). In 1893, Freud was still writing of "the agency of somatic innervations" (p. 40) and "physical modification corresponding to [inhibited intentions]" (p. 44). Up to that time, neurology had regarded the disorder as organic. With Freud's removal of hysteria from this organic realm, the problem-solution was revolutionary in its formulation.

Hysteria can be regarded as a persistent anomaly which refused to respond to neurological treatment, and for which the new germ-

theory class of formulation was unsuccessful. Neurology's eagerly anticipated breakthrough, in which hysteria would be demonstrated similar to general paresis and treated on an organic basis, refused to occur. Freud moved to a radically different solution from his early acceptance of biological etiology, one which was based on his technical therapeutic innovations, drew from a different framework than neurology, which in turn created its own framework.⁴

Freud came to regard hysteria as a disorder of the cognitive/emotional sphere, with symptoms as meaningful as the symbolic expression of ideas and emotions "forgotten" and operating at a not-conscious level, which were related to forbidden material from early in life. This formulation needed obviously, the ideas of the unconscious, of repression, of psychological trauma and the genetic principle to provide continuity from the past to the onset of symptoms, which Freud later developed. With this set of concepts accompanying the original insight that hysteria was psychological, Freud's solution had reached full revolutionary status, with all that implies regarding the usual reception of revolutionary formulations in Kuhn's schema.

Strachey reported (1955, V. II, p. XV) that Studies on Hysteria "was unfavorably received in German medical circles; it was, for instance, very critically reviewed by Adolf von Strümpell, the well-known

⁴ For an interesting argument as to where Freud's perspective originated, see D. Bakan's Freud and the Jewish Mystical Tradition (1958).

neurologist." This makes a good deal of sense, particularly that latter point that an eminent neurologist should 'pan' it. In Kuhn's schema, it is precisely the group for whom the paradigm is revolutionary, that is most threatened and resistant, with some of its members never adopting the new paradigm. Freud's resignation from professional associations was requested, his public presentations were poorly received and his colleagues were confounded and alienated.

Subsequent importance of interpretation in the psychoanalytic DM. The technique of interpretation gained enormous importance in psychoanalysis. As previously mentioned, the classical paradigm was undisputed and relatively unchanged until approximately 1914. After this point, Freud, and others, made some major changes in the DM with two results. First, some of these changes made by others induced the development of differentiable DMs within the psychoanalytic rubric (e.g., Kleinians); secondly, there remained a group of analysts committed to the original, classical paradigm. Thus, for instance, they eschewed work with other than neurotics, and they regarded interpretation as the analyst's ultimate (and for some of the more dedicated- the only legitimate) tool. While interpretation was important to all the analysts, it was the lodestone for these classical or interpretational analysts.

Freud (1937)⁵ differentiated between interpretation proper, such

⁵ Freud (1937) *Constructions in analysis; Collected Papers*, Vol. V. Cited by Loewenstein, 1958, p. 209.

as those of a dream or a parapraxis, and a "genetic" interpretation or reconstruction of a remote and repressed event or fantasy, or an impulse or reaction which remained repressed but active. Loewenstein continued the differentiation among types of interpretation which he regarded as the "specific tool of the psychoanalyst..."(p. 205), in 1958 (two years after the inception of the double-bind paradigm). Two years before the double-bind, Stone (1954) was identifying interpretation as one of the "technical instrumentalities...which is ultimately relied on for the distinctively psycho-analytic effect."

We would, while acknowledging that other psychotherapeutic agents play an important role in the psychoanalytic process, assign to interpretation the unique and distinctive place in its ultimate therapeutic effect. We would, I think, require that the interpretations achieve this effect through the communication of awareness of facts about himself to the patient, with the sense of emotional reality that comes only with technically correct preparation, rather than through certain other possible effects in the transference counter-transference system which occur so frequently in other psychotherapies. (Certainly, they occur also in psychoanalysis, but they are regarded as miscarriages of effort). (Stone, 1954, p. 574).

Comparing Loewenstein or Stone's views with other analysis reveals that they are actually rather flexible in their orientation. For instance, Loewenstein (1958, p. 208) includes confrontation and clarification as important, "preliminary to interpretation." In other words, they are willing to give a role to other verbal techniques. Other analysts of that same period, however, are considerably more inflexible. Eissler (1953) regarded the ideal case as one in which interpretations alone are used and prove sufficient; any other activities

on the part of the analyst are termed 'parameters' and by implication, stand as occasionally unavoidable, if unfortunate, modifications of technique.

Analysts of this period, whether flexible or inflexible with regard to interpretation clearly belonged to the mainstream of psycho-analytical development and represented the dominant forms of conceptual framework and clinical practice. They worked almost exclusively with neurotics and based their formulations in direct evolution from Freud's early classical paradigm and DM.

The problem of narcissism. Narcissism constituted a problem relatively early in psychoanalysis for a number of reasons directly related to the paradigm and the role of transference and interpretations and working through. Part of the almost exclusive occupation with neurotics by this dominant group was related to the fact that Freud's formulations had been based on hysteria, then extended to phobic and obsessional neuroses⁶ -- all of which were regarded as transference neuroses and thus all amenable to the same type of dynamic

⁶Freud and Breuer's formulations of hysteria were in 1892 and 1893, culminating in 1895's Studies on Hysteria; Freud, however, had an article published in 1894 (The Defence Neuro-Psychoses in Neuroligisches Zentralblatt, Nos 10 and 11) in which he connects with "hysteria, many phobias, and obsessions," in fact signifying that "observation of these [phobic and obsessional] patients had resulted in a contribution to the theory of hysteria (1955, Vol. I, pp. 59-75). Henceforth, these were regarded as transference phenomena.

formulation and the same operative processes in analysis, i.e., transference, interpretation, and working through. Thus, there were clear practical and theoretical reasons for remaining with transference neuroses. There were also theoretical reasons -- most importantly, Freud's 1914 paper on narcissism. By the third phase of the paradigm set, the strength of positive transference was regarded as crucial. When the positive transference had developed to "a sufficiently strong attachment, the treatment [was] in a position to prevent all the more of the patient's repetition-actions and to make use of his intentions alone..." (p. 373). This point was highlighted by those patients who were perceived, by Freud and then others, of being incapable of forming this strong transference reaction because of a preponderance of narcissism; this was the case with schizophrenics and particularly borderline personalities.

A pressing motive for occupying ourselves with the conception of primary and normal narcissism arose when the attempt was made to bring our knowledge of dementia praecox (Kraepelin) or schizophrenia (Bleuler), into line with the hypothesis upon which the libido theory is based. Such patients... display two fundamental characteristics; they suffer from megalomania and they have withdrawn their interest from the external world (people and things). In consequence of this latter change in them, they are inaccessible to the influence of psychoanalysis and cannot be cured by our endeavors. (1914b, Vol. IV, p. 31).

Schizophrenic patients were perceived as having withdrawn their attachments to the external world, without replacing them by phantasy substitutes, and as such were incapable of forming either the essential transference relationship, or concomitantly the repetitions in

the analytic situation, which of course would preclude any psychoanalytical cure as the change agent would be excluded; narcissism would also preclude any control by the analyst of the compulsion to repeat outside situation.

The combination of the historical precedence of the transference neuroses, then the differentiation between transference and narcissism in object-choice, proved crucial in the development of the psychoanalytic DM. As has been mentioned, most analysts remained focused on transference neuroses, and emphasized interpretation as the primary therapeutic technique; this group can be regarded as the direct successors to the early Freudian paradigms. Not all the analysts, however, perpetuated the focus on the paradigmatic phenomena.

Anomalies in the Traditional Psychoanalytic Population

Rapid extension of paradigm to new phenomena. Relatively early, clinicians began to apply psychoanalytic insights and techniques to very different phenomena. Freud himself had made some extensions, notably to obsession and phobias, allowing insights gained during the application to modify his hysteria formulations. As all these disorders are easily dealt with by a transference formulation, Freud's extension served to elaborate and refine the paradigm and DM. His 1914 article on narcissism can be seen as an attempt to demarcate those clinical phenomena for which he felt psychoanalysis was appropriate and could be held accountable, and those for which it could not, and therefore

could not legitimately be faulted for difficulties in the application. With regard to this point, of course, he was entirely correct; there is no reason a framework should be faulted when it was applied by others to phenomena for which it was not constructed. Yet this is exactly the sort of extension that took place, one which the Kuhnian framework essentially predicts -- the extension of paradigm or analogue to new phenomena:

...it was still early in the history of psychoanalysis that Abraham began to treat manic-depressive psychosis [1927], and not too long before Simmel [1929] opened a psychoanalytic sanatorium where he treated very severe neuroses, incipient psychotic conditions, and addictions. Also early came the psychoanalytic interest in character, beginning with Freud himself [1933], followed by the distinguished contributions of Jones and Abraham. However, character analysis as a special technical problem was precipitated sharply into the foreground of general interest by Wilhelm Reich's brilliant and stimulating, although still controversial book [1947]. With Anna Freud's book on The Ego and the Mechanisms of Defense [1946], one might say that a movement toward the broadening and multiplication of the psychoanalytic spheres of interest in the personality, and an appropriate complication of psychoanalytic technique, found general and secure acceptance. (Stone, 1954, pp. 567-568).

By 1919, what was referred to as borderline neuroses and psychoses had been addressed (Clark) as had dementia praecox (Kempf); both were treated within a modified psychoanalytic therapy. Aichhorn had adapted the traditional technique to treat delinquents and Anna Freud in the twenties began formal analysis of children (1928). Federn's work with psychosis implied further technical modifications. The extensions proliferated to the point that Anna Freud began to be concerned about the distribution of effort (1954, pp. 610-611).

For years now, our most experienced and finest analysts have concentrated their efforts on opening up new fields for the application of analysis by making the psychotic disorders, the severe depressions, the borderline cases, addictions, perversions, delinquency, etc., amenable to treatment. I have no wish to underestimate the resulting benefits to patients, nor the resulting considerable gains to analysis as a therapy and science. But I regret sometimes that so much interest and effort has been withdrawn from the hysteria, phobic and compulsive disorders, leaving their treatment to the beginners or the less knowledgeable and adventurous analytic practitioners. If all the skill, knowledge and pioneering effort which was spent on widening the scope of application of psychoanalysis had been employed instead on intensifying and improving our technique in the original field, I cannot help but feel that, by now, we would find the treatment of the common neuroses child's play, instead of struggling with their technical problems as we have continued to do. (A. Freud, 1954, pp. 610-611).

Interestingly, her recommendation appears to be, not a return to the former spheres of application, but an increase in the number of psychoanalysts. "Let us hope that the future analysts, who occupy our Training Institutes now as candidates, will be numerous enough to spread their energies over both fields." (p. 611).

With the extension of the paradigm to new clinical phenomena, a number of anomalies emerged and eventually challenged the integrity of the Freudian paradigm. Similarly, in the enormous literature and practice in the traditional clinical phenomena, anomalies also arose. Those anomalies that engendered the sense of crisis immediately prior to the inception of the double-bind family therapy paradigm are relevant to the present purposes and so will be reviewed here.

Unsolved problems of individual psychoanalyses. Anomalies in the

traditional psychoanalytic population emerged gradually and slowly, as treatment of neuroses gained a history. Contrary to expectation, it was not the case that one group drew attention to an anomaly, while another group proposed a solution or accomodation; rather, the same individual or group, would point out an anomaly and at the same time propose a solution.

One of the most notable analysts in this regard was Clarence Oberndorf. His awareness of problems arose in two areas (the second of which will be reviewed later in the discussion of anomalies). He proposed,

that it become a custom endorsed as good practice that a case be reviewed in consultation if the patient has been under classical psychoanalytic-treatment 4-5 hours a week for more than, let us say, three hundred hours. Such groups [of 3 experienced analysts] might reach an opinion as to whether the case should continue with the same analyst, be discontinued as not well adapted to psychoanalysis and some other method attempted, or be referred to a second analyst because of transference difficulties leading to therapeutic stasis. (1950, p. 403).

Oberndorf was here addressing himself to the increasingly obvious situation that psychoanalytic treatment results had not always warrented the time, money, and effort put into them. He reviewed the two foremost psychoanalytic journals (The International Journal of Psychoanalysis and The Psychoanalytic Quarterly) for the preceding decade and found that "practically no articles have been devoted to the results of psychoanalytic treatment and very few deal with therapy directly. One of the most recent books on technique also fails to mention results, except in fragmentary cases and the question of unsatisfactory results

is not considered." (1950, p. 394).

The unsatisfactory results took a number of forms. One was the presence of a "distressing recurrence of illness in treated patients" (Gralnick, 1962, p. 517); this, of course, should not have occurred according to the psychoanalytic formulation of therapy and change. If both catharsis and recollection had transpired, these processes would insure the neurosis (with respect to the particular conflictual material treated in the analysis) would not recur. And apparently, the frequency of relapses was greater than one could assume to be the case if they were explained by incomplete analyses.

Alternatively, the analysis may have been technically successful, but the person's social relations remain as bad as before analysis. Facetiously speaking, everything in the patient is cured except his human relations. Or in the phrase of one analyst, "On completion of analysis, the patient is wiser, but sadder and lonelier." (Ackerman, 1954, p. 362). Or perhaps more painful yet, patient and analyst may vary in their opinion regarding success of treatment, or the

results might be entirely unacceptable to relatives and associates close to the patient. Results in which all are concerned (the patient, his environment, and the physician) are content with the outcome are frequent. On the other hand, there are many instances when none of these participants in an analysis is satisfied..." (Oberndorf, 1950, p. 395).

These situations constitute anomalies because they counter either psychoanalytic practice or theory. For instance, though analysis certainly does not promise happiness, it does directly imply greater

understanding of one's needs and motivations, as well as greater control over one's behavior in relation to significant others (by the working through of the repetition compulsions via the transference). The above difficulties clearly counter this direct implication. Which is not to indicate that all difficulties which arise are anomalies; for instance, Stern (1938, p. 475) discussed a type of intellectualizing patient as a technical difficulty.

It is among these patients [those convinced they are inferior individuals] that one frequently finds (the bane of the analyst's existence) those who get a thorough psychoanalytic education through being analyzed and remain quite sick people. They have the intellectual equipment to accumulate knowledge and unless the analyst is on his guard, will use this knowledge not to unravel sources of their feelings of inferiority, but neurotically to bolster up their ego, with pseudo-therapeutic results.

This sort of difficulty is not anomalous as it can be accounted for, easily, by psychoanalytic practice or theory. For instance, it can be said that such patients, though recollecting and free associating away, have maintained the dissociation of intellect and affect, and as such, have undergone little or no catharsis. As such, they cannot be said, with the framework, to have been successfully analyzed. The solution lies in technique, that is, how to induce the patient, probably through deprivation, (of reality markers, of response to overtures, etc.) to couple recollection with appropriate affect.

Bona fide anomalies however, continued to emerge. Such were the situations in which the identified patients became healthier, and a family member reciprocally, became "sicker." This state of affairs

was not always obvious since analysts eschewed contact with family members, but it did occasionally reach report (and was subsequently documented as a frequent occurrence by family therapists). For instance, within the child-guidance context, which used psychoanalytic theory and practice, such reports had begun publicly by at least 1942. Burgum reports that,

Occasional murmurs about the father as a factor in child guidance treatment echo through the field. The fact that the mother is the person most involved in responsibility for the child and his difficulties, and also most accessible in terms of her own time and agency working hours, tends to focus attention on her both in diagnostic and treatment considerations. The father is not entirely neglected, yet the full significance of his role in the treatment situation is rarely adequately realized. (Burgum, 1942, p. 474).

What makes the situation particularly significant is that the treatment was in the traditional individual model (with mother seen as adjunct), (p. 481), and the reciprocities in health followed a general pattern (implying the situation was not idiosyncratic).⁷

⁷The pattern was one in which the presenting problem was construed as mother's difficulty in controlling a young child, with accompanying mutual antagonism and the child's possible destructive behavior, poor school adjustment or irritability. Father

"on the other hand,...either lurks in the background at time of referral or comes forward as the child's champion [and] has taken on the role of child's protector. If the mother requests placement, the father asks that an attempt be made to work out the problems in the home...He comforts [the child] against the mother's abuse, becomes his last refuge, and sometimes takes over, under strict matriarchal surveillance, a large share of the maternal case." (p. 474).

Neurotic complementarities. Finally, a series of articles regarding the protraction of neuroses among marital couples appeared. Again, it was Clarence Oberndorf who appears to have initiated this set of concerns. In 1934, he published an account of his treatment of "a case" of folie à deux, wherein he reformulated some of the traditional ideas regarding this "sharing" of psychoses. The term "folie à deux" had traditionally been used to refer to psychotic manifestations shared by two people.

Generally, it is applied to the so-called induction (contagion, infection) of a second person by a primary mentally sick person living in close proximity. The inductor suffers from paranoid ideas, excitements or depressions which are transmitted to the second person. Those who are induced, are usually blood relations (siblings, or parents and their children); less frequently the second member of a married couple. This circumstance is interpreted by Bleuler to 'throw light on the familial disposition to the disease.' (Oberndorf, 1934, p. 14).

His reformulation is to the effect that: first, the tendency for folie à deux to appear in blood relations is less due to "constitutional predisposition" than to the "possibility for early and intense identification of children with parents to each other" which especially affects "ideal formation and self-appraisal." Secondly, Oberndorf highlights the presence of two initially neurotic conditions, which complement each other, and develop "mutual symptomatology" only after marriage.

It is a matter of frequent observation that when the induced person is removed from the inductor the symptoms in the former disappear. Exception to this result has been noted, which again led Bleuler to comment that the second person sometimes suffers from an independent disease in which only certain manifestations, such as the content of the delusions, are determined through induction. I believe this to be the case far more frequently than it is possible to demonstrate

in advance folie à deux. In the case of neurosis à deux which I present, the pre-existent individual neuroses could be thoroughly established; the mutual symptomatology developed only after marriage. (Oberndorf, 1934, p. 14).

It can be inferred that this case may have sparked Oberndorf's interest in complementary neurotic processes in couples, as he addressed this pattern repeatedly and some of his conclusions presage his later writings.^{8,9}

⁸ e.g.,

In the latter [folie à deux] we have the psychological interplay of a group of two. In nearly all of the more permanent and stubborn neurotic compulsions one finds that the condition is often nourished by a lesser neurotic tendency on the part of the individual principally involved in the patient's neurosis. At times the attitude of domination or indulgence of the patient by the complementary person may be so patent that the layman correctly interprets the patient's father, mother, etc., as the main cause for the continuity of the neurosis. (Oberndorf, 1934, p. 15).

⁹ Oberndorf's perceptiveness regarding family dynamics is remarkable. In 1934, at a time when essentially no one was writing of current family dynamics, Oberndorf had the prescience to address them repeatedly. For instance,

In pathological interlocking familial situations often the only reason why the one person comes for treatment and the other does not, rests in the circumstances that the sick person is consciously struggling to break the abnormal situation which he unconsciously desires, whereas the person who is considered well desires consciously to maintain it, without necessarily appreciating its pathological aspect. From the strictly technical interpretation of induced psychopathology, the inductor in neurotic familial situations is often the individual who considers himself and is often considered the normal person. This leads to the very frequent comment on the part of psychiatrists dealing with childhood psychiatric problems that

Oberndorf then discussed his analysis of each member of nine couples, both individuals of which eventually entered analysis with him. He reported on nine such cases in twenty-five years practice. He stated that after varying periods of time, the spouse would enter analysis, in several instances because improvement in the first member (first in analysis) would convince the second person that s/he must change for the marriage to continue.

His article is fascinating in part because of his style of presentation. Published in 1938, when perhaps the press for space in journals was not so great as it is today, Oberndorf's account includes many pages of case history and comparative information which makes for a vivid picture of the processes he was trying to explain. Also his initial paragraphs are careful, methodical arguments, paving the way to his slightly heretical material; he began with "The theory of the psychoanalytic school attributes the origin of neuroses to a conflict between conscious strivings and unconscious longings or to a wholly unconscious conflict of purpose." (p. 453), and proceeded to Oedipal aspects of development, occasional inability to relinquish parental attachment, and later difficulties in the psychic or sexual life of the adult. All

the parents need to be treated rather than the child.
(Oberndorf, 1934, p. 16).

The only other instance encountered during reviewing this literature, of such perspicacity regarding family dynamics is Kempf's 1919 article on psychoanalytic modification in the treatment of psychoses (cited later with regard to anomalies in non-traditional psychoanalytic populations).

this was preamble to his contentions that marital difficulties, particularly sexual problems and infidelities, were often the result of the transposed incest prohibitions, that is of the persistence of incest prohibitions after marriage that have been "transferred" from parent to the spouses precluding guiltless sexual contact between spouses. He believed that the degree to which the Oedipus complex had waned in both partners was the single most important psychological factor influencing the success, or failure, of a marriage (1938, p. 464).

As determinedly psychoanalytic as his 1934 presentation was, there are clear passages that presage his later work on neurotic complementarities. For instance, while discussing "conscious feelings of inferiority in women...associated with the castration complex (penis envy)...", Oberndorf comments that

This type of woman with a psychological masculine urge may find herself powerless to refrain from attempting to assert leadership over her husband at critical moments in connection with generally accepted masculine prerogatives. In consequence the man may feel himself humiliated and react with a refusal to be subjected by his wife or to acknowledge his own proclivity to feminine subordination. If he is unable to defend his masculinity or to find assuaging compensations for his latent femininity, he may seek escape in divorce. He often rationalizes his flights with such reasons and causes as incompatibility of temperament, social inequality, or difference of religion. The above mechanism was operative in the wife of one of the couples reported by the author (1934, p. 16).

Of course, marriages are well known where mutual inversions of minor degree have happily complemented each other. This mechanism was operative in one of the couples reported as "Folie à deux." (1934, p. 15).

He considered the technical problems "knotty" but not insurmountable.

With regard to transference, he wrote that these patient's should be treated as any other; with regard to each patient using the analyst as a source of resistance, the analyst should maintain his position of neutrality with utmost inviolability.

Bela Mittelman, citing Oberndorf's work with couples' neurotic complementarities, began writing in the 40's about what he termed "neurotic circular interpersonal reactions." (1944, p. 480). In a study of fifteen couples, Mittelman reconstructed the complementary neurotic reactions by which "partners followed an intrapsychic vicious circle of reactions which they acted out in an external vicious circle" (p. 483). (Interestingly enough, Mittelman was discussing here, a couple in which "...as a result of psychotherapy, the patient's behavior suddenly changed, the partner felt that he was now really being abandoned and punished for all his past aggression. He reacted by cutting his wrist..." (p. 483) (which can be seen as a variant to reciprocity in health anomaly!).

It was his contention that because of the continuous and intimate nature of marriage, every neurosis in a married patient was "strongly anchored in the marriage relationship" and that the presence of a complementary neurotic reaction in the partner contributed to the protracted nature of the patient's neurosis and should be addressed in treatment. An example, for instance of such a complementarity is the

common pattern of an attempt at self-sufficiency through emotional detachment on the part of one partner (usually the man), and an intense, open demand for love on the part

of the other (usually the woman). When the woman's violent demand for love and support arouses the man's fears, he becomes more detached while she evaluates this detachment as a humiliating rejection. Her guilt and fear of abandonment keep pace with the violence of her demand. Concomitantly, the man, who is warding off his desire for dependency and submission, becomes afraid of being completely dominated by these excessive demands for affection and defends himself by increasing his detachment. At the same time this detachment is an expression of his anger toward her, aroused by his interpretation of her resentment and criticism as a frustration of his own need to be loved. The complementary reactions are further overdetermined by the fact that both partners project the guilt from their mutually aggressive attitudes and blame each other for their difficulties. (Mittelman, 1944, p. 484).

(Mittelman provides several more such patterns but one will probably suffice at this point.)

Mittelman stated that in treatment he analyzed only one partner of each couple, though he interviewed each partner at least twice and occasionally gave "weekly psychotherapeutic interviews", i.e., not analysis. The analysis of a partner, when necessary, was invariably conducted by another analyst (p. 491), though he cites Oberndorf's concurrent analysis and recognizes the advantages to knowing in detail of each partner's reactions. Mittelman emphasized that "he must be careful to limit his activity to analytic interpretations and avoid taking sides or rendering judgment on the qualities of either mate" (p. 490), demonstrating himself to be both wise, but orthodox with regard to the role of interpretations. If the patient's analysis was successful, the complementary reaction of the partner often subsided without any sort of direct treatment (p. 490); and if the complementarity was not

of a serious nature, it was often enough to explain to the patient, using daily material, how his/her behavior affected the partner (p. 489).

In a later (1948) report, he considers the advantages of problems of concurrent analysis of twelve couples. Each partner received analysis for four of the couples, and for the remaining eight couples, one partner was analyzed and their partners received between two and twenty sessions of intervals from one week to several months; it should be emphasized that treatment was concurrent, not conjoint, that is, each individual in the couple was seen, but the couple was not seen together as a couple. Couples were adjured not to "mix" their analyses (p. 193).

He felt that the primary advantage in this mode of treatment was that both the realities and the neurotic interactions of the partner's behavior became clear.

Current reactions of dependency, guilt, hostility, anxiety, and superiority are revealed in a clearer light, and at times one of the mates gives information about crucial trends in the other mate. These trends may be so underplayed by the other mate that they would not otherwise be adequately recognized by the analyst, although their investigation is imperative for the success of the treatment...Simultaneous treatment of married couples was successful in eleven of twelve instances, including two which ended in divorces satisfactory to both parties. In four of the twelve couple, both mates were analyzed, in eight, one mate was analyzed, the other received briefer psychotherapy. (Mittelman, 1948, pp. 196-197).

"The treatment" here, it should be understood, refers to the analysis of the primary patient; the treatment might well be directed to the two partners, but only in the service of breaking the complementarity and allowing the primary patient to begin working in analysis.

By this time, Mittelman was experienced enough in concurrent analysis to provide indications, contraindications, and technical pointers to the reader. It is also clear, however, that he was construing this as an adjunct to treatment, i.e., as a preliminary and/or "extra" treatment designed to rob the complementarity of its effects and allow the patient to get on with analysis. All the formulations were psychoanalytic, as was the treatment format.

The efforts of Oberndorf and Mittelman can be regarded as those of psychoanalytic adherents who recognized problems arising in the treatment of neuroses by classical analysis, problems which could not be accommodated by traditional practice, which was based upon the intimacy of individual analysis and a careful putting aside of reality factors. Ignoring reality issues was necessary, technically -- to enhance the transference, and theoretically -- as neuroses were caused in the past and could be cured only by addressing the past, whether through recall, abreaction, or working through repetitions in the transference. The presence of contemporaneous, reality factors which decidedly influenced neuroses, and which at times were amenable to advice, constituted an embarrassment, and an anomaly. Classical theory was unable to explain why simply direction and reflection of behavior and reaction could change the patient's behavior and his or her spouse's. Contemporaneous events and relationships were exerting enormous influence over the neuroses of patients in a way psychoanalytic theory could not account for, nor deal with in the traditional manner.

As this became more obvious, greater effort was directed toward the problem areas, and the rate of publications addressing them accelerated. During the 30's Oberndorf was virtually alone among analysts in dealing with neurotic complementarity and couples; Mittelman had then extended Oberndorf's work in 1944, 1948, and 1952.¹⁰

In 1953, P. Martin and H.W. Bird published "An approach to the psychotherapy of married partners: the stereoscopic techniques" in which they advocated the close cooperation between the analysts or therapists who are each treating the individual partners in a marriage. The expected regular and frequent contact between the therapists was regarded as leading to a more accurate perception of the marital relationship. This, of course, is an extension of Mittelman's position, wherein he saw both members of a couple to better assess their neurotic reactions to each other. Though his focus remains essentially intrapsychic (he speaks of neurotic reactions rather than relationships) his approach does show clear relational elements. The shift to a relational focus is clearer still in Martin and Bird. (It should be noted that they emphasize the relationship between therapists must be friendly and informative for the stereoscopic technique to work) (cited in Grotjahn, 1960, pp. 39-40).

¹⁰ Significantly, in a laudatory and affectionate obituary written at Oberndorf's death in 1954, his work outside the mainstream of psychoanalytic theory and practice is not once mentioned, though his mainstream activities, of which there were many, were cited and discussed. It can be inferred that the writer of the obituary and editors of the Psychoanalytic Quarterly were uncomfortable enough with that material to exclude it completely.

Then in 1956 Alexander Thomas reported his work with eight couples, each partner of which was treated in one or two weekly sessions to about 150 sessions per partner. (At no time did both partners of any couple meet simultaneously with Thomas.) He points out that the therapist should

Behave as if he knew nothing about the other individual... No guarantee for a solution should be offered...It must be made clear that each partner will have to resolve his or her neurotic functioning in the marriage in order to make a go of it...[and] in simultaneous psychotherapy, the transference situation between patient and therapist tends to become secondary and the emphasis to shift from the therapeutic situation toward the marriage relationship. (Grotjahn, 1960, p. 44).

Martin Grotjahn's modification. Some of the work of Martin Grotjahn may be said to culminate this trend toward technical modifications in classical clinical practice. Grotjahn developed in greatest detail, the techniques for using "marginal interviews", i.e., interview of the patient's spouse, to further the analysis of the patient. He is quite clear that these marginal interviews are an adjunct to "the therapy", that is, the analysis of the individual patient, and information gleaned from them is used in the service of the individual analysis (1960, p. 172; p. 238) which is regarded as the basis of psychotherapy (p. 9). There is no pretense of couples or family therapy. His theory remains strictly psychoanalytic, though his practice bends the clinical rules. For instance, he discusses not reaction complementarities but "family neuroses," and states that "there are cases in which seeing the marriage

partner is strongly indicated for sound analytic reasons..." (1960, p. 169).

He began seeing marital partners when, doing the training analyses of analytic candidates he found that the

favorite defence of many physicians...is isolation of... insight from emotional experience...[which] frequently renders their training analysis therapeutically ineffective. They learn everything and change nothing. I found that to involve a physician's marriage partner, directly or indirectly, and under favorable circumstances, was an effective way to combat this fateful tendency...and I decided that this technique might also be applicable to the treatment of other resistive patients with similar defenses...(1960, p. 162).

These and subsequent interviews were planned to "reinforce, stimulate, or to safeguard therapeutic progress" in the patient's analysis. Occasionally marginal interviews led to "collateral psychotherapeutic-treatment with regular interviews" (p. 169), but, it appears, not to concurrent analysis. If analysis of Grotjahn's partner were indicated, that partner would be referred to another analyst (1960, p. 236); in fact, Grotjahn would not accept in analysis, the partner of a patient even if treatment had been terminated, so that if re-analysis were required, he would be free to accept back the former patient (1960, p. 236). It was made clear that he had never conducted "the simultaneous analysis of two married partners." (p. 233) At other times, marital partners were referred to group psychotherapy (Klein, 1965, p. 26). "In certain emergencies" (p. 295) or "in rare cases" (p. 179) a family conference may be called; however, it is clearly an extraordinary measure and one that is isolated and not part of a series.

Grotjahn was at pains to point out that his work was psychoanalytic, and involved a "new direction in analytic technique" (p. 279), but not theory. He pointed out that

the emphasis is on the analysis of the individual neurosis as anchored in the complementary neuroses of the family... This method employs the main analytic concepts of transference and resistance, of interpretation, lifting of repression, insight, and final interpretation. In my opinion family therapy is analytic therapy although its approach differs from the standardized formal analytic approach described in textbooks of psychoanalytic technique. (p. 275)

His formulations revolved around the ideas of complementary neuroses which were anchored in the family collective unconscious and the family neurosis (p. 272). The family collective unconscious was comprised of the projective extensions of both the conscious and unconscious. For instance, the marriage neurosis (between marital partners and not children, depending upon presence or absence of children or their ages)

may be defined as the transfer and projection of unresolved, unconscious conflicts from the parts of both partners into the present; that is, from children of families into the marriage situation. As long as the neurotic unrealistic aspects of this transfer remain unconscious, the prognosis is gloomy. In a neurotic marriage, the bond repeats old infantile patterns. The female may unconsciously see an image of her father or brother in her husband. On deeper levels she may also see in him her mother or sister, a circumstance which complicates matters endlessly. A man may easily realize he represents his wife's father -- it is a different matter for him to understand he has been cast in the role of his own mother-in-law. (pp. 92-93).

While clearly psychoanalytic with regard to formulation and change processes (he explicitly deals, for instance, with interrupting repetitions and beginning new learning in marriages) (p. 213), Grotjahn's

work in practice was clinically far enough beyond the pale by a summary work in 1960, to draw criticism from dominant psychoanalytic circles.

"Psychoanalysts like Edward Glover, Karl Menninger, and Leon Saul more or less warned against [analysis of the family neurosis] as dangerous or detrimental to a pure transference neurosis." (p. 281) Kubie is cited (pp. 68-69) as thinking it unwise for the same analyst to conduct the analyses of both marital partners simultaneously because that one or another of the partners will lose confidence in the analyst's impartiality.¹¹

A third set of criticisms from the more traditional psychoanalysts concerned the perceived destruction of the one-to-one relationship of classical treatment. This was regarded as the basis of analysis and its disruption was ascribed to attempts to correct countertransference difficulties in the analyst him- or herself.

Finally, the analyst's "unresolved" countertransference difficulties may be criticized more personally.

It may be hinted darkly that he has an unresolved interest in watching the primal scene. He may be accused of having a "papa complex," or of attempting to become the pater familiae, who God-like guides his flock of sheep. He may

¹¹ Grotjahn dealt quite nicely with another obvious problem here, the emergence of paranoid ideation, by requiring either separate analysts at the first sign of paranoid reaction, or planning a "joint family interview" of patient, partner and analyst. He felt that this technique made paranoid distortions and misinterpretations less "malignant, although it cannot prevent argument and misinterpretation. It safeguards the sanity of the sane family member by giving him some orientation in the reality of the treatment." (p. 273)

be accused of a great unconscious need to play the omnipotent, omniscient, all-powerful, God-like-father-mother. Because of the dependency phobia of our time, he may even be suspected of trying to enslave whole families. The perfect psychoanalyst will thus be contrasted with the image of the imperfect, depreciated, and deplorable psychotherapist whom we meet in the funny papers and mystery shows. There is no doubt that such therapists do exist; but this cannot be avoided in a time of transition, when well-organized training is limited, and above all, slow. (Grotjahn, 1960, pp. 276-277)

Grotjahn's rejoinder was that by dealing exclusively in counter-transference consideration critics were ignoring the presence and role of the complementary family neurosis, which had its own structure and dynamics, and required analytic conceptualizations and treatment. Psychoanalytic family treatment was regarded as stimulating the process of working through, especially in those cases that tended to isolate analytic insight from affect. (p. 274)

What is quite obvious is that a sufficient number of analysts had bolted from the mainstream, so that they, in some sense, constituted a threat. These criticisms can be regarded as attempts to discredit the increasingly influential techniques on theoretical and personal grounds, or to redefine the techniques as "not psychoanalysis" and thereby maintain the purity of the DM-"purity" here, not being meant perjoratively, but rather in the sense of "uncontaminated" or "wrong-to-the-spirit" of the framework. (The double-bind DM will be observed to attempt the same type of probably necessary maneuver and Freud's 1914 paper may be seen similarly). This would seem to be an almost inevitable development

as members of a DM, with all that implies in perception and self-identity attempt to keep the set of commitments in which they have invested, stable and consonant with their total framework of commitments. It is an attempt to correct what are seen as faulty thinking, as well as an attempt to keep the ground from shifting out from under one's feet. The process is an attempt to stave off a sort of betrayal from a DM that is beginning to deteriorate from within.

Grotjahn's work with the family neurosis represents the most developed response within psychoanalytic conceptualization, to the anomalies emerging from within the traditional clinical populations of psychoanalyses--the transference neuroses. He addressed the resistant cases (resistance with regard to isolation of affect, to lack of change in treatment, to relapse) and bent the techniques without breaking fundamental rules. It was a highly developed, and usually successful (and therefore threatening) accommodation of the psychoanalytic framework. Ackerman's work, though psychoanalytic in origin, steps firmly and unambiguously outside the classical psychoanalytic framework, both in technique and conceptualization. While Grotjahn regarded the individual as the patient, based formulations and change in psychoanalysis, Ackerman's work treated the whole family, with its dynamics, and redefined the change process into family terms.

Two further points regarding implicit aspects of Grotjahn's work that presaged the best of family systems therapy should be mentioned. First, he was exquisitely aware of family homeostatic aspects, and

particularly the reciprocity of health previously mentioned. For instance, while discussing the possible uses, including diagnostic, of marginal interviews, he states, "As the patient progresses, this family neurosis will also change, and this change will involve and perhaps endanger the mental health of his mate and the future of the marriage." (p. 156) Similarly, in the previously mentioned issue of paranoid ideation in the spouse, his technique served delicate homeostasis functions.¹²

The second such point relates to Grotjahn's recognition that contemporary family processes served maintenance functions with respect to pathology. Oberndorf and Mittelman had, of course, dealt with this in some measure because of their interest in how neurotic complementarities served to protract neurosis in the analytic patient. However, their emphasis was on unconscious constellations that fit in a complementary manner in the initial stages of relationships (and often influenced object choice) having set up marital neuroses, allowed/induced them to continue; also, both wrote about the presence of individual neuroses in the partners, antedating the relationships. Grotjahn's work emphasizes this aspect also, but had frequent undertones addressing the processes whereby pathology was actively maintained in family behaviors and words;

¹²Also see pp. 171-172 for extended discussion of spouse's difficulty in integrating 'relationship' to spouse in view of spouse's changes and meeting the therapist.

this connoted a shift in etiology from intra-psychic dynamic processes to interactive, communicationally-induced pathology. For instance,

Analytic psychotherapy of the family neurosis is based on an important insight: a person's neurosis may be anchored to a large extent in a complementary neurosis involving his marriage or his family. The unconscious communication or interaction between people can cause and maintain a neurosis, as another kind of communication can help to cure it. The family may thus both help and hinder growth and maturation. (Grotjahn, 1960, p. 282)

and,

The therapist of the neurotic family must understand that a neurotic reaction in one person may complement neurotic behavior in another; that a neurosis (or psychosis) may be the only possible adjustment of a child to a neurotic (or psychotic) family; that the way in which symptoms develop in one person may be the result of unconscious clues and orders given by another member of the family. In these situations, psychoanalysis of the individual patient may remain incomplete and may fail unless the analyst treats the family neuroses, in addition to the neuroses of the individual. (Grotjahn, 1960, p. 289)

If put to the test and asked whether neuroses are "caused" by interaction between people in the family of marriage (not origin) or by psycho-dynamic processes from family of origin, it is obvious that Grotjahn would take a modified psychoanalytic position. Nevertheless, the direction of his flexibility is prescient.

The influence of contemporaneous relationships on neuroses and on the course of treatment constituted an anomaly primarily of technique, and also had some implications for the paradigm. These difficulties were anomalous because psychoanalytic technique and theory of change demanded treatment in isolation from the world (as mentioned previously,

particularly with respect to enhancing transference). By ignoring reality factors and remaining a blank screen, the analyst fostered the necessary frustration, regression, and development of transference, through which the neurotic repetitions could be worked through. To take into account reality factors, particularly with regard to relationships, would drastically interfere with the development and use of that therapeutically essential "relationship," the transference. Thus, with regard to practice, with patients involved in neurotic complementarities, the analyst was caught between the devil and the deep, blue sea. To take into account the reality relationships hampered the transference development and thus the therapy; to not take them into account hampered the therapy because little or no change came about.

The anomalous aspects with regard to paradigm concerned the therapeutic processes of change, and not etiology of neurosis. Psychoanalytic theory could rather easily deal with even neurotic complementarities; for instance, it could point to neurotic involvement in object choice or indicate that both partners had been neurotic prior to their relationship. The existence of contemporaneous influence over neuroses did not contradict the etiological notions of origin in the past.

The unsolved problems of individual psychoanalysis, then, did not constitute anomalies with respect to theory of etiology, but were anomalous for both technique and theory of change, both of which had had paradigmatic status in the framework.

CHAPTER IV

ANOMALIES ARISING WITH THE EXTENSION OF THE PARADIGM TO NEW CLINICAL POPULATIONS

The success of the psychoanalytic DM with the transference neuroses impelled practitioners to attempt applying it to clinical phenomena other than the transference phenomena. Psychoanalysis was applied to dementia praecox/schizophrenia, to psychotic depressions, latent schizophrenia or borderline personalities, and to the psychopathology of children. Upon application, it became quite clear that major reformulations would be required as neither technique nor theory could accommodate aspects of these psychologically different clinical syndromes.

This set of anomalies proves particularly important for two reasons. First, the extension of a successful paradigm-DM to phenomena for which it was not constructed, is another confirmation that Kuhnian analysis is appropriate, with respect to the sequence of developments preceding the inception of family therapy. Second, the anomalies that arose from this extension were more marked than those that arose from traditional clinical populations; the discrepancy between DM-expected, and encountered processes, proved just that much more noticeable.

Several authors had drawn attention to the fact that the framework was largely unsuccessful when extended too far beyond itself. For instance, Oberndorf, only six years before the publication of the D-B theory, stated that,

It is my impression that the proportion of unsatisfactory

results may have increased with the extension of the psychoanalytic method to include cases with marked schizoid personalities or schizophrenia, as well as those with extremely weak or uncertain superegos ('psychopathic personalities'), and with the use of the method of physicians whose natural talents are unsure or training is incomplete. (Oberndorf, 1950, p. 396)

Similarly, attention has been drawn specifically to extension to clinical phenomena outside the originally intended realm of consideration, "...instances where the psychoanalytic method has yielded unsatisfactory results [include]...intractable psychiatric conditions for which the psychoanalytic method was not originally intended but is now frequently used." (Oberndorf, 1950, p. 396)

Interestingly enough, Don Jackson, one of the double-bind originators and himself psychoanalytically trained, made note of this process.

Another influence toward family studies, which has indirectly stemmed from the psychoanalytic movement, has to do with the disappointment in the results of this expensive and time-consuming technique and the possible relation of results to a change in the type of clinical material with which psychoanalysts deal. The shift in emphasis from symptom neuroses to character, marital, and child guidance problems has resulted in a broadening of analytic techniques with an emphasis on parameters and on psychoanalytically oriented psychotherapy. (Jackson, 1961, p. 33)

Perhaps the finest example of awareness regarding both the results of extension and the response of analysts comes, again, from Oberndorf (1950, p. 397).

Psychoanalytic treatment of the more complicated schizoid, paranoid depressive, or extremely narcissistic personalities may have continued three to five or even more years by the same or successive analysts. The unsatisfactory results in

this group are discouraging and arouse doubts in the mind of the analysts as to whether the inadequacy lies in limitations of the method or in his own skill in applying it.

These cases particularly suggest research to determine more accurately the efficacy and scope of psychoanalytic therapy; also to judge earlier the type of personality likely to respond favorably, the suitability of the analyst to the patient, and to assess promptly the patient's difficulties. The latter fall into the realm of transference, empathy, their derivatives and corollaries, which are of paramount importance. This is always true whether the therapy aims to make the deeply repressed unconscious conscious, adapt the individual to cold, cruel reality, integrate the id with the ego and the superego, or also, I presume, in the procedure seeking the goal of 'orgastic release.'

For this reason the attention of most investigators dissatisfied with their own results has centered about the question of technique with the hope that improvements in technique, especially the analysis of the transference, would bring about better results. (emphasis added)

Anomalies that Arose from the Treatment of Children

The extension of psychoanalytic treatment to children occurred quite early. Anna Freud was presenting publicly in the 20's (1926) and citing Aichhorn's work (with delinquents) during the second decade of the century (1954, p. 607). It is Parloff's opinion that it was in the area of child analysis that the first serious questioning of the basic assumptions of psychoanalysis occurred. Grotjahn apparently agrees, stating that for many years, Oberndorf's work relating to family neurotic patterns remained unnoticed.

Then, slowly, some analysts started to report techniques

which differed from those their colleagues had reported in Glover's¹ questionnaire. These were the child analysts. In their efforts to analyze children, they constantly had to deal with problems caused by the help or hindrance of the parents. (1960, pp. 28-29)

Parents of young patients. The issue of dealing with the effects of parents in treatment and with the parents themselves was quite difficult, and actually, could not be accommodated within the psychoanalytic framework. The injunction against seeing relatives forbade it technically, and theoretically; meeting with parents contaminated the emergence of the transference relationship. Moreover, particularly with children of latency age, meeting with parents was regarded as potentially disrupting any trust the patient might invest in the analyst.

On a theoretical level, mechanisms of psychoanalytic treatment were involved. The progress of therapy depended upon the elicitation and working through of the transference neurosis and repetition compulsions. The analyst, to facilitate such a transference, attempted to be a "blank screen" and introduced few reality constraints into the situation; this included providing only minimal personal information about him/herself and also not meeting with the patient's relatives, either to give, or receive information. Success in treatment depended upon elicitation and working through of transference. Introduction of disruptions, such as meeting one or more family members could impede either

¹Glover, E. Techniques of psychoanalysis: New York: International Universities Press, 1955.

of these processes (Parloff, 1961). At the time of writing, Parloff stated that it was still common practice for analysts to refuse interviews of family members on the assumption this would interfere with treatment.

In spite of these knotty theoretical points, it became increasingly clear that parents had to be included in the analysis of children--they were wreaking havoc on the treatment and their effect had to be mitigated. Melitta Sperling (1949) devoted an article to dealing with parental, but especially maternal, anxieties, restrictions, and subversions of analysis of children with psychosomatic problems. Similarly, Parloff (1961, p. 41) drew attention to

the mounting clinical evidence..that parents..were..so bumbingly diabolical. The patient's mother appeared to have the remarkable knack of being able single-handedly to produce neuroses, psychosomatic syndromes, psychoses, and even juvenile delinquency with equal facility and on either side of her ambivalence.

Such remarks strongly suggest that theories of etiology were failing, and that mounting frustration on the part of clinicians was encouraging a scapegoating of parents, and particularly the mothers of disturbed children.

Reasons for including parents, even peripherally, into the child's treatment began to be noticed.

Child analysis failed to fulfil its initial promises as analysts discovered that even five one-hour sessions a week could not keep up, in most cases, with the influence of the remaining 163 hours at home. The number of child analysts who have stuck to their last is surprisingly small, and this fact must have had some

influence in giving tacit approval for others to seek new techniques in treating children. (Jackson and Satir, 1961, pp. 33-34)

Moreover, since the child was perceived as the victim of the parent's conscious or unconscious "malevolence," the treatment of the child could be enhanced by also treating the parents (Parloff, 1961, p. 41). Also, the child guidance clinics (which almost without exception operated within the psychoanalytic framework) were finding that in a significant percentage of cases, children were referred with problems which were directly, and often dynamically linked to marital problems, and that treating the child alone was fruitless, and similar in structure to treating one individual in a neurotic complementarity. (This sort of finding is, it would seem, partially responsible for the transformation of some child-guidance clinics to family-therapy clinics).

Work such as Burgum's (1942, previously cited) regarding the father in relation to child and mother's treatment, indicated that treatment of child and mother were not enough.

Cognitive structure of children. The necessity of working with parents was damaging enough to the integrity of the application. Quite as damaging was the suggestion that, not only was psychoanalysis not very effective with children, but that child analysis and its formulations, were not always appropriate to the children themselves as a change process.²

²Dr. Alvin Winder has noted that apparently Fritz Redl found the psychoanalytic change processes compatible with those children he treated, and was able to use the framework successfully.

Auerswald (1972, pp. 87-88) relates his discovery of this shortly after having finished training as an analyst.

Armed with the paraphernalia of a "child therapist"--family dolls, dart guns, etc.--I found myself in various rooms with various black ghetto children doing 'play therapy.' I discovered at that time that I had a problem. Nothing I did changed much of anything. The kids I was trying to "treat" were delighted to shoot mother "symbolically" ad infinitum. They were supposed to respond to my interpretations by entering into a process. They didn't. They just shot mother. Furthermore, they shot father, brother, sister and Jesus Christ with the same glee, depending upon which stimulus I placed before them. Frequently, they shot me...I began to understand that the reason I could not treat "delinquent" ghetto kids with play therapy was to be found in their general cognitive organization vis-a-vis my own and the frame of reference I was using, and not in the structure of psychodynamic defense systems which, in that particular group of kids, existed at best in only rudimentary ways.³

Carl Whitaker (1972, pp. 98-99) too, became frustrated and discouraged when no change developed. Carolyn Attneave (1972, pp. 122-125) within the context of delivering comprehensive care to children (not within the psychoanalytic framework, though her experience is similar to the others in the problems she was responding to) started doing family therapy "because nothing else made sense, although we'd not heard of it as a separate field."

³In case we should consider the case of delinquent children too extreme an extension to adequately judge psychoanalysis's efficacy, we should recall that the first extension of the framework to children was by Aichhorn, to the same population, that is, delinquent ghetto children.

Anna Freud's work, as early as her 1926 report also took into account the differences in the cognitive structure, and existential situation of the child. She stressed that adults came to treatment, whereas children were brought for treatment; it was necessary, therefore, to form an understanding or to reach a consensus with each child patient about the reason for treatment. This agreed-upon reason was always in terms the child could understand and was usually, it appears, initially formulated by the child. Though she did not elaborate explicitly to any great degree, it is obvious from her reports that Freud took pains to establish a working relationship with each child; she was explicit about the need to establish a working relationship that is different from that with an adult patient. What she was less than explicit about was that the pains she took to establish these relationships were directed toward a type of relationship that depended as much upon its affective component as any interpretations to effect change. The differences between adult and child in cognitive structuring were taken into account primarily in her technique rather than in her theory.

Initial efforts to see the young patient's parents. Eventually, social agencies dealing with children, and to a far lesser degree, child analysts, began to see family members, though "only as the backdrop against which to view the individual" (Sherman, 1961, p. 14). There developed a definite rift such that some therapists were advocating seeing family members, as a family and not in concurrent but separate analyses, whereas others

advocated a more orthodox psychoanalytic approach. For instance, Berta Bornstein in 1948 contended that contact with parents should not go beyond mild and supportive adjunct psychotherapy and the analyst should not use dynamic or genetic interpretations with the parents.⁴

We can assume that the rift became recognized in part because there were enough advocates of both positions to mount an explicit campaign. By the early 1950's, caseworkers had had enough experience in seeing relatives of patients that they were able to identify points of difficulty in their new endeavor.

The trouble has been, as I have already mentioned, that the theory underlying casework practice with individuals has not been supplemented with the methodological equipment necessary for the understanding and treatment of the family constellation. It is an interesting phenomenon--supplying grist for the sociologist's mill--that studies and experimentation of the family as a group and on family pairs, conducted by psychiatrists, psychoanalysts, caseworkers, and anthropologists, have been reported in the literature only recently. Caseworkers have begun to seek new operational hypotheses on the basis of their own experience and have also extrapolated related concepts from the findings reported by other professions. (Mitchell, 1961, p. 72)

By 1954, Nathan Ackerman's work had enabled him to elucidate these gaps more fully, (though it would be another four years before publication of his theoretical solutions).

In child guidance practice, the problems of treating the parents of disturbed children have not been solved. There

⁴Cited by Grotjahn (1960, p. 30).

have been many failures. We have not yet succeeded in formulating adequate criteria for the psychotherapy of parental role. In examining the causes of failure of treatment of mothers of disturbed children, several factors loom large: the complexity of the definition of mothering; the difficulty of relating the dynamics of individual personality to the mothering role; incomplete or incorrect diagnosis; vague and changing orientation to goals with resulting confusion of the therapeutic course; failure to properly integrate the treatment of child and mother; failure to understand the parental conflict and the fundamental interdependence of maternal and paternal functioning; and finally, the failure to relate the therapy of child and mother to a total psychosocial evaluation of the family as a unit. (Ackerman, 1954, p. 362)

Anomalies in child analysis. If we recall Masterman's point, that anomalies arise because the paradigms have been pushed too far, and Kuhn's point, that anomalies are obdurate discrepancies between DM predictions/expectations and what actually occurs, then two situations here can legitimately be termed anomalies. First, the failure rate in child treatment if parents were excluded signalled the first anomaly. For, if change depended directly upon the transference relationship, and if contact with family members hindered development of transference, the analyst was caught in a bind. S/he could either see, or not see, the family members; if s/he did, transference development was hindered and change jeopardized, yet if s/he did not, then experience had shown change was hampered in the child. Secondly, the differences between adults and children in cognitive capabilities dictated the necessity of changes, both clinical formulations and practice to de-emphasize transference, interpretation, and many verbal techniques; thus Anna

Freud set about establishing an emotional working relationship with the child patients, and Melanie Klein and her group split off from classical analyses in part because of using play⁵ rather than talk with young patients. Thus, the techniques were definitely different than in Freud's paradigm; the mechanism of change, working through the repetitions, continues to apply, except that transference itself is no longer involved.

Ackerman's response to psychoanalytic-child anomalies. Nathan Ackerman's work with families constituted the first psychoanalytic family treatment publicly reported; that is, his focus was not on the individual but on the family itself as the unit of conceptualization and focus for change. He had served for several years as a consultant to a social agency primarily oriented to the treatment of children, which eventually dissolved to form a Family Mental Health Clinic (Ackerman, 1961a, p. 228). Ackerman was concerned about the treatment of children in the light of anomalies mentioned here. In 1950, he and Sobel challenged the traditional psychiatric diagnostic categories for children and set out to define the children's personalities as a function of the "sociopsychological configuration of the family unit." (p. 744) Because their unit of definition was the child in his/her interaction with significant others, they

⁵Dr. Al Winder has brought attention to this important split from the classical psychoanalytic framework.

decided that the treatment of the young child should begin with the treatment of the family group. They observed, however, that no one was doing family treatment, and the conceptual framework for it did not exist--in fact, they stated that, "We do not know whether it is in fact possible to treat families as groups. Perhaps it is not possible." (p. 745). They then proposed a methodology for the study of the pre-school child with the context of the family (p. 745).

By 1954, concern primarily for the child had shifted to the integration of the family unit with the child. Ackerman pointed out many of the difficulties in adjunct treatment, where child "and someone" would be seen (pp. 361,366). He also began to elaborate what would be his primary interest for the remainder of his work

finding a better way of conceptualizing the interrelations of illness in one person with psycho-social processes of the family entity. This immediately involves a consideration of three interrelated phenomenological levels: what goes on inside one person; the make-up of that person as it is expressed in adaptation to specific family roles; and the structure and function of the family as a group entity. (Ackerman, 1961b, p. 256)

In 1954, he again pointed out that an appropriate frame of reference had not yet been devised to integrate the therapy of an individual with the therapy of a family group (pp. 367-368).

His Psychodynamics of Family Life: Diagnosis and Treatment (1958) constituted his initial paradigmatic statement where he provided that framework for the integration of individual and family he had talked about.

In this book he offered a comprehensive theoretical approach in which the emerging personality of the individual is related to the family configuration. He presented a systematic scheme for organizing and correlating data on the family group with data on individual family members. This scheme encompassed bio-psycho-social factors, patterns of communication, control of conflict, mechanisms for restitution, extrafamilial social roles, pathogenic conflicts, stricings and values, capacity to accomodate to new experiences, reality testing, learning and growth. (Mitchell, 1961, p. 73)

The object of the diagnostic family process was to pinpoint the central conflicts of the family group, and their corresponding role disturbances. However, both diagnosis and treatment focused on the family as a whole, rather than the concommitent treatment of several members in separate analyses.

...if we are to understand the individual, we must also understand the structure, function, and vital processes of the group as a discrete system. It is for this reason that, in this past decade, "family diagnosis" has been coming to the fore as a focus of interest, replacing an interest in "family-oriented" diagnosis. It represents a shift to viewing the distress of the individual as less the problem than a symptom of the problem of pathology in the whole family. Family diagnosis is oriented to "the client in the family" and their reciprocal interplay; it replaces the separatism expressed in the phrase "the client and his family." One cannot overemphasize the basic difference in orientation produced by the substitution of the word "in" for the word "and." The "in" orientation is holistic; and "and" orientation atomistic. These differing orientations reflect differences not only in personality theory but also in practical family analysis. (Sherman, p. 18, in Ackerman, 1961) [his emphasis].

Ackerman also responded to a number of the anomalies that had arisen with respect to child analysis, and re-interpreted them according

to his dual-focus individual-family system.

From a clinical point of view, several considerations ought to be stressed. First, mental illness is contagious. It is passed from person to person. If we test this idea, we find some interesting things, some that ought to have been obvious. Families are rare in which only one member is psychiatrically disturbed. Where one is disturbed, one inevitably finds other members of the same group also suffering a psychiatric disorder. These illnesses may differ but it is hard to know whether the first person that comes to our attention is the most sick or the least sick. There is, too, a very important ongoing interaction between the psychiatric sickness of one member and the psychiatric sickness of another, where the two are intimately bound in their day-by-day family experience. In other words, there are complementary relations between the illnesses of respective family members who share the problems of daily living. The one individual who happens to get to us first, the so-called primary patient, ought to be viewed as one link in the distress and disablement; but we must also examine the ways in which his disturbance represents a symptomatic or functional expression of the emotional warp of the family as a whole. Often when one looks into these matters does one find that one part of the family maintains a tolerable emotional balance at the expense of another. That is, if one person is to keep his head above water, to maintain at least a tolerable functioning without breaking down, it can sometimes only be done when another member of the family is made sick or kept sick, as is the case with some forms of depression. In a tacit, covert way, other family members behave in a manner that induces the depressed person to stay depressed. If we intervene and relieve that depression, we upset the pre-existing emotional balance in the family relations, and someone else cracks up. Now, this is generally what one finds in family groups where there is some degree of cohesiveness, some partial complementarity among the members, so that the family functions are carried on. Despite this apparent unity, on a deeper level, the family is emotionally divided into competing factions. In some families there may be open warfare between one part of the family and another. The fate of such internal war influences the susceptibility to breakdown and the outcropping of psychiatric illness. (Ackerman, 1961a, pp. 233-234)

Ackerman's work was clearly the most sophisticated form of response to the child anomalies arising from within the psychoanalytic framework. Many family therapists of a certain ilk credit Ackerman with having "started" family therapy--others discount his contributions as adaptations/extensions of psychoanalysis and refuse to regard his conceptualization and methodology as family therapy. It is unclear at present if his work was revolutionary or evolutionary in Kuhnian terms; in any case, the influence of his work remains powerful among many groups of family therapists, who it should be noted, can be regarded as antithetical in practice and theory to the D-B adherents. D-B adherents claiming priority of revolution, however, cite the incontrovertible fact that the D-B paradigm reached public report two years before Ackerman's paradigmatic work.

Anomalies that Arose from the Treatment of
Borderline Personalities or Latent Schizophrenia

The development and awareness of a "pseudo-neurotic" masked psychotic syndrome. A surprising amount of attention was paid to borderline personalities or latent schizophrenia (also known as pseudoneurotic schizophrenia or borderline psychosis) beginning, with any frequency, during the 1930's. "Surprising" because, the syndrome is not exactly one that seems likely to attract the attention of analysts. For example, it seems to make sense that schizophrenia should attract their attention, both in terms of treatment and research; if psychoanalysis was largely

successful for neurosis and nothing seemed particularly successful with schizophrenia, the obvious next step is to bring the theory and practice of psychoanalysis to bear on schizophrenia--to cure or ameliorate, and to gain information about the disorder(s). However, even the existence of a borderline syndrome was not known until the practice of psychoanalysis led to its discovery/formulation as a clinical entity. While schizophrenia was a manifest psychopathology, latent schizophrenia was exactly this: latent. It became a recognized syndrome, not through nosological observation or because of theoretical predictions; but rather, was uncovered, then highlighted, by a psychotherapeutic process. The reason for analysts' attention to borderline syndromes rests, not in the intractability or inverted prestige of schizophrenia, but in the contingencies of everyday private practice. As the number of analysts grew, and concomitantly, the number of cases treated, there began to appear an alarming number of patients who appeared neurotic (of one form or other) when accepted into treatment, then, during the exigencies of analysis, decompensated into frank psychosis. Even allowing for the inevitable percentage of mis-diagnoses in early treatment, poor treatment leading to exacerbation of clinical picture, and "hidden" psychoses, the number of analytically precipitated psychoses in neurotic-appearing patients began to concern the profession. This was particularly problematic as most analysts dealt (and still do) with out-patients; precipitation of psychosis in one's out-patient is anxiety-provoking, to say the least, with its subsequent need for

hospitalization and dimming of prognosis. A literature search reveals a larger body of writings on these borderline personalities in the 1930's and 1940 than on the treatment of schizophrenia, though both contributed their own anomalies to the rising sense of crisis.

Though the 1930's heralded the intense work on borderline issues, Clark addressed himself to what he termed the "borderland neuroses and psychoses," as early as 1919, commenting that,

I think very few physicians have seriously used psycho-analytic methods in treating the essential neuroses without sooner or later making an attempt to employ the same method in the borderland neuroses and psychoses, with varying results. (Clark, 1919, p. 306)

Briefly, he reported his work (beginning as early as 1912) with several sets of syndromes "not ordinarily classed as belonging to the analytic type of psychoneuroses." (p. 306) These included dementia praecox.⁶

While Clark would use psychoanalytic interpretations in what appear to be efforts to understand the patient's difficulties, he felt that

...under no circumstances should we really attempt to require the patient himself to get that insight or attempt to act upon it as such. In other words, dementia praecox should not be analyzed, but by a method of conscious suggestive therapeutics and rationalization

⁶With the dementia praecox cases (most of which were advanced by time of referral to him), he was of the opinion that any attempt at traditional psychoanalysis "invariably does harm," as it takes away the "crutches of formulations these patients have made by which they can get on with the realities of their existence. They are then reduced to actual impotence." (p. 307)

the praecox individual may be helped to an adolescent sublimation of work and recreation short of the adult demands of emotional maturity. (Clark, p. 308)⁷

It is obvious that Clark had used the term "borderland" to denote ambiguity in clinical picture; later use of the term refers to a specific syndrome which should be regarded as a fairly stable personality integration, which has specific characteristics, one of which is the potential for temporary psychotic decompensations. The term does not refer to a process of transiency nor does it connote anything like "almost psychotic" or "nearly psychotic;" the majority of individuals or borderline personality type at no time experience psychosis.

Greenacre (1941), in a series devoted to the predisposition toward anxiety, emphasized increasing the reality hold of the patients and strengthening the ego through "education" of the patients' narcissism; she emphasized minimizing acting out and concessions to the patient's demand for activity.⁸

Stern in 1938 drew attention to the reason for regarding borderlines as a group by themselves, as differentiated from both the psychoneuroses and the psychoses (p. 488), though he takes pains to point out that his presentation is unavoidably vague, as the syndrome is neither a variety

⁷In some respects, Clark's formulations bear resemblance to Harry Stack Sullivan's a decade later.

⁸reviewed by Stone, 1954.

of neurosis or psychosis (and hence he is elucidating a syndrome without the familiar psychoanalytic processes and characteristic clinical patterns his readers were accustomed to) and because his efforts were early in the process, and more time and investigation were required (pp. 487-488). In part, Stern's difficulty was attributable to the presence of processes not accounted for within the psychoanalytic framework making the borderline syndrome both anomalous and important. His efforts were particularly directed toward diagnosis as, it should be recalled, diagnosis of borderlines was heretofore difficult and crucial to the profession so that they would not be involved as patients in psychoanalytic treatment.⁹ His initial set of statements give a good indication of the perplexity with which this phenomenon was viewed, and also the sense of failure, crisis and need for revision.

It is well known that a large group of patients fit frankly neither into the psychotic nor into the psychoneurotic group, and that this border line group of patients is extremely difficult to handle effectively by any psychotherapeutic

⁹Stern (1938, p. 468) listed clinical symptoms to help the practitioner recognize a borderline personality early in treatment, in time to terminate and avoid the significant potential of a psychosis, or to make technical modifications in the treatment. Stern's list includes: 1. Narcissism; 2. Psychic bleeding--i.e., psychic collapse at trauma rather than resilience; 3. Inordinate hypersensitivity--i.e., easily deeply insulted or wounded; 4. Psychic and body rigidity--'The rigid personality'; 5. Negative therapeutic reactions--i.e., depression or suicidal attempts; 6. What looks like constitutionally rooted feelings of inferiority, deeply imbedded in the personality of the patient; 7. Masochism; 8. What can be described as a state of deep organic insecurity or anxiety; 9. The use of projection mechanisms; and 10. Difficulties in reality testing, particularly in personal relationships.

method. What forced itself on my attention some three or four years ago was the increasing number of these patients who came for treatment. My custom was not to treat them analytically, except when they were suffering acutely from neurotic symptoms (i.e., anxiety, depression, etc.) and required immediate therapy. With these I tried the usual analytic therapy but in the large majority of the patients, after a more or less lengthy course of treatment, I had to stop treatment leaving them not much benefited. In the case of the 'neurotic character,' which makes up a very large proportion of this border line group, much more often than not I attempted no treatment at all, for the simple reason that I had learned from experience that our knowledge of analytic therapy as employed with the psychoneurotic patients was insufficient to achieve good results with this group, especially when their suffering was not acute enough to justify immediate therapy. (Stern, 1938, p. 467, emphasis added)

His perception of the failure of classical psychoanalysis for these patients is reiterated throughout his article (pp. 468,469,488). Specifically, though he induced and allowed the thorough working through of the object libidinal material "they nevertheless remained sick", unlike usual neurotics (p. 468). The mechanism of change in classical analysis, working through the repetitions within the transference, took place, but were not effective for this group of patients. This clearly runs counter to paradigm-induced expectations and, as such, constitutes an anomaly.

Stern was to eventually decide that though the disturbed psychosexual impulses were operative and must be included in treatment, the presence of disturbed narcissism, and not psychosexual difficulties, were the cause of borderline states (pp. 488-489). These patients showed the presence of narcissism to a degree not present in transference

neurotics and "it is on the basis of narcissism that the entire clinical picture is built." (p. 469) This clearly, is a significant departure from the Freudian paradigm with respect to formulation of the disorders. Stern is positing a new root cause for this disorder, then goes on to further differentiate the borderline group from the psychoneurotic. With borderline patients, there is an immaturity and insecurity in the transference not seen in neurotics (p. 478); it is a transference of "extreme dependence" (p. 480). This is related to another point of differentiation; that is, the anxiety of borderlines (which is quite intense and from which the symptoms arise as defense) arises from the early infantile period, and as such, at an earlier developmental stage than neurotics (p. 487). Finally, a significantly greater proportion of the ego functioning is involved in the disturbance, which of course contributes to the increased difficulty in treatment and the "more grave" prognosis (p. 489).

Now, this is all rather interesting as, it might be recalled, the reason for psychoanalyst's not treating psychotic manifestations involves the inability of these patients to form transferences. This was discussed at some length by Freud in his 1914 paper on narcissism. In that paper, he deduced from his paradigm and DM that the presence of significant narcissism precludes the development of transference and hence, the change processes of working through. Contrary to this rather important point, Stern and others, began to note that borderlines, though characterized by inordinate amounts of narcissism, did in fact form

transferences. Moreover, these transferences were particularly intense, primitive, dependent, and vivid.

Because of the intensity and dependency of these transferences, Stern (1938, p. 479, footnote) recommended two technical modifications: a greater degree of supportiveness by the analyst and "a rather constant occupation" with diminishing the intensity of the transference. These modifications are in direct contradiction to the psychoanalytic practice of fostering the transference. The presence of transference among patients with marked narcissism clearly constitutes a discrepancy between paradigm and DM induced expectations and empirical findings; as such, transference among narcissistic patients constitutes a theoretical anomaly with respect to the libido theory. The technical modifications required to deal with borderline transferences constituted a technical anomaly with respect to the classical paradigm.

His final conclusions were that narcissism was indeed amenable to not only psychoanalytic investigation, but also treatment (p. 488), though it was emphasized that the technical modifications required were substantial.

Zilboorg (1941) addressed the behavioral picture of what he termed "ambulatory schizophrenics" (another term at times used for the borderline types) which captured the quality of their lives rather well, and which mentioned many of the elements later discussed by Otto Kernberg (1975). This includes the "outstanding feature" of the inner life of these persons.

...they are literally suffused with hatred. They are hardly ever free from its pressure, during their waking hours or in their sleep. This hatred they may not express, but it appears under two guises. As a rule the combination of both usually presents itself in one person: first, they are usually tense, almost to the point of being constantly aware of physical tension, of "being tied up in knots"; and second, anxiety, of which they may on occasion be aware, makes its appearance, but even when these persons realize that they are anxious, they perceive it not as fear, nor as anxiety only, but as an inner, violent, helpless anger, even rage. (1941, p. 149)

Zilbourg documents the paucity of friendships, dissociation of affection from sexual life, propensity for suicide, assaultiveness, and homicide of those (ambivalently) close to them, and their impulsivity. His report, he felt, needed more time to be less sparse, as not many ambulatory schizophrenics had entered treatment, or remained.

Federn published a three-part series on the psychoanalysis of psychoses, where he addressed himself to diagnosis, treatment and dynamic processes in psychosis; (most of that work will be reviewed with the schizophrenic anomalies, except for some material on latent schizophrenia more appropriate here). It was his feeling that in cases of latent psychoses, one wanted to make the diagnosis as soon as possible, to avoid beginning or continuing analysis, and that this is what Freud had in mind when he advocated trial-analyses (1943, p. 15). Four years later, Federn reiterated this concern, in the interests of preventing a latent schizophrenia from becoming manifest; though not always possible, it was to be attempted as the prognosis of schizophrenia is graver than that for latent schizophrenia and always unpredictable.¹⁰

¹⁰ He also "sharply separated" early childhood schizophrenia from latent schizophrenia, a necessary point, in view of the occasional confusion between the two (1947, p. 132).

He addressed himself in 1943 to exactly the phenomenon of concern among analysts--how to diagnose the latent schizophrenia early enough to terminate analysis and hopefully avoid an analytically-precipitated psychosis. As such, he focused on latent schizophrenic indications that occurred during analysis, particularly: the patient's intuitive acceptance without resistance, of interpretations regarding symbols and primary processes, and quick or even sudden disappearance of severe neurotic symptoms.¹¹

Unfortunately, the presence of these two features led analysts to think the analysand was an excellent analytic candidate; hence, the relatively large number of precipitated psychoses. In doubtful cases, Federn strongly advised trial-analysis with immediate interruption if the indications were present (pp. 41-42). He apparently felt that even at that late date, 1947, the unwitting precipitation of psychoses was continuing and not being recognized (1947, pp. 138-139):

No latent schizophrenic should be "cured" of his neurosis, and he definitely should not be treated by the standard form of psychoanalysis. For thirty years cases have come to me for treatment or for consultation after having been naively, and apparently well, psychoanalyzed. Their (correct)

¹¹Other warning indicators included: a history with different levels of neuroses such as neurasthenia, hypochondria, early conversion hysteria, obsessions, anxiety hysteria and severe depersonalizations (hence the term "pseudo-neurotic schizophrenia"); psychotic periods with true delusions and loss of reality testing in early childhood; lasting deterioration at work and isolation in social contacts after puberty or after leaving home or school (as neurotics tend to temporarily improve with change in circumstances); prevalence of a narcissistic reaction pattern over that of object libido choices; and typical physiognomic signs in posture, look, and gesture (which Federn does not, however, specify). (pp. 15-16)

diagnosis was neurosis. During all that time the latent schizophrenic state was not recognized. Seldom did the psychoanalyst either anticipate the outbreak or acknowledge, after it had occurred, that it was his interference that precipitated the manifest psychosis. He would invariably think the case was too difficult for psychoanalytical treatment. This kind of error is not a personal one, but one made by "standardized" psychiatry. [his emphasis]

Awareness of crisis in the late 1940's and early 1950's. By 1950, the realization of difficulties with borderline personalities had become more acute. Bychowski (1950, p. 407) referred to the "growing interest in the problem of therapeutic failures" and addressed the borderline group, who he characterized as superficially appearing to have made a "rather good adjustment, with only occasional behavioral deviations and mood swings until this facade collapses 'either due to a dramatic event in their life, or to the removal of their Ego defenses as in a training analysis...'" (1950, p. 409). His paper focused on the concept of ego weakness, i.e., an ego that could not be relied upon when stressed to continue to differentiate "self" from "other", and reality from fantasy. Bychowski also considered and required technical modifications which emphasized active reduction of transference. This was accomplished by avoiding the "classical analytical reserve" (p. 413), by stressing reality testing (p. 414) and the inclusion of the "total reality" of the patient in the analysis--that is, events from work, relationships, behavior in the "outside world" and projective mechanisms (p. 414). Adherence to the classical position of enhancing transference

and reducing the reality "markers" was perceived as dangerous in its simultaneous elicitation of disturbed tendencies (in the transference) with decreased controls, (the control of acting-out tendencies, impulsivity and intensity of transference being very important in the treatment of borderlines). Bychowski's modifications in treatment can be said to loosen with one hand and protect or restrain with the other, the narcissistic libido and the ego, in the service of rectifying the narcissistic disturbances while strengthening the ego (1950, p. 416).

This working through of primary narcissism will then form an indispensable basis for productive sublimation of narcissistic libido, a process whose vital necessity cannot be overestimated. Since investment of the immature Ego with an unusual quantum of primary narcissism seems to be an important factor of its weakness, we cannot strengthen the Ego without freeing it from this load. On the other hand, however, we know that a proper amount of narcissism is an important prerequisite of the Ego strength (Nunberg, Federn). Moreover, redirection of primary narcissism toward productive and realistic objectives, seems the best, and perhaps the only preventive measure against future relapses as a result of a clash between reality and unbound narcissism. (p. 415)

This is clearly not classical psychoanalysis and posits different change processes as well as different technical processes. For instance, he even advocated the temporary interruption of treatment on occasion to allow the strengthening ego to consolidate certain of its gains (p. 417).

In a later paper (1953), also on borderline phenomena, or latent psychosis, Bychowski addressed diagnosis, dynamics and therapy. (He discussed psychological testing to aid diagnoses and the problem of divergent opinion between psychologist and analyst, in either direction. A year earlier, L. Zucker had devoted an entire and quite good article

to diagnosis and dynamics of latent schizophrenia based on Rorschach studies (1952). Zucker also addressed the advisability of diagnosis to avert psychosis. Briefly, he emphasized its existence as a clinical entity (p. 484), but one which was usually masked by character-neurotic difficulties, deviant behavior (delinquency, perversion, addiction), or psychopathy, which upon provocation "may burst into psychosis"; he paid particular attention to those psychoses provoked by psychoanalysis whether therapeutic or didactic (pp. 485, 499, 500).

Bychowski also posited a formulation of the dynamic structure of latent psychosis that departed firmly from Freudian formulations and appears related to the ego psychoanalysts (a large and important group in psychoanalysis but one which is tangential to present purposes). Bychowski states that in the course of early development, a discontinuity occurs,

so that early ego states remain untouched under the cover of later ego formations. Accordingly, archaic constellations remain fixated and preserved, as it were, for future reference. They form then the psychotic germs which, under the impact of various dynamic and environmental factors, can cause the psychotic breakdown of ego defenses and sever whatever reality contact and testing have been built up in the course of later development. (1953, p. 491)

This formulation takes into account the periods of time with non-psychotic functioning, the relatively rapid decompensation (as he's not suggesting an accretionary process, but a break-through), the presence of characterological and neurotic signs (serving to keep the psychotic materials in check), as well as the peculiar rigid and brittle quality of individuals subject to these processes. This formulation provided the basis, and

rationale, for further modifications in technique; Bychowski reiterated his 1950 modifications and added several elements. He became more emphatic about the need to introduce modifications to strengthen and protect the prepsychotic ego (pp. 500 and 502).¹² Reality testing grew in importance as the ego became more exposed to "the bombardment of the repressed id derivatives" (p. 502). The frequency of sessions and position of patient (recumbent or seated, facing or not facing the analyst) were factors used to control reality testing also. Similarly, interpretations needed to be spaced, and careful. "Too deep and too rapid interpretations, especially when not accompanied by certain reassuring explanations, may expose the ego to the onrush of id impulses as well as to the implacable sadism of the superego", (p. 500); some resistances were left uninterpreted and free association minimized as it encouraged both regression and looseness of thinking.

The primitive transference relationship could show either infantile leaning and oral dependence with derivative primitive identification or, negatively, in defensive hostility culminating in destructive rage...

¹²

His point regarding the importance of the weak ego for psychoanalysis appears well taken. Over twenty years later, (1974), Strupp's report on the Menninger Foundation study of psychotherapeutic success includes two (of five) concluding statements about ego strength:

1. A high level of Initial Ego Strength represents a good prognosis for all forms of psychoanalytic psychotherapy, but especially psychoanalysis.
2. Patients with ego weakness (especially "borderline" cases) frequently fail to benefit from psychoanalysis or supportive psychotherapy... (p. 273)

The dissociation between these various attitudes of the ego toward the analyst as a temporary love-hate object may be so blatant as to make for a true split in the object relationship. In this way, in the transference, the ego repeats the cleavage by the archaic ego which in its deep ambivalence had split parental images into bad and good objects. It is of great therapeutic value to work through this peculiar situation and to demonstrate it to the patient with absolute clarity. (p. 501)

The focus of treatment was also shifted from analysis of libido conflicts to the analysis of primary narcissism and its defensive "archaic megalomania" (p. 502)

All of the preceding modifications of formulation, technique and focus resulted in a sophisticated and effective form of treatment for the borderline personality type, but clearly by 1953, its most sophisticated proponent was well beyond the psychoanalytic pale. Bychowski had had to abandon the paradigmatic technique of free association, the change processes of abreaction, and alter the working through of transference, eschew interpretation as the primary technical tool, change the emphasis of treatment from verbal technique toward a verbal-relational model,¹³ and even abandoned the traditional recumbent position of the patient. Though Bychowski clearly regarded this sort of treatment as within a psychoanalytic context, the psychoanalytic community was not undivided in its appreciation of such work (as will be seen later).

¹³For instance, when discussing the patient's mounting anxiety, Bychowski states that interpretation must "be combined with firm reassurance and...active kindliness [which] should make it clear to the patient beyond any doubt that he can count on his analyst under any circumstances." (p. 501)

Jacobson in 1954 discussed the "indistinct but convenient" term of borderline, which epitomized common features in patients that displayed,

ego distortions and superego defects, disturbances in their object relations, and a pathology of affects beyond what we find in common neurotics. For this reason they usually need many years of analysis with slow, patient, consistent work in the area of ego and superego, with great attention to their particular methods of defense and to their affective responses in which these defenses find special expression. This work is so difficult because such patients call into play auxiliary defense and restitution mechanisms which impair their reality testing to a greater or lesser extent, engaging at the same time the outside world, and in particular the significant objects for the purpose of their pathological conflict solutions. For these reasons they may require modifications of our usual technique, which neurotic patients do not need. (pp. 596-597)

Finally, Stone in 1954, while discussing the widening scope of indications for psychoanalysis, discussed the importance of early diagnoses (p. 589) and considered them to be increasingly in evidence and in fact added, "It is a long time since I have treated an actively psychotic patient; borderline cases and severe character disorders have been numerous." (p. 581, footnote). He reviewed this experience of the transference of borderline patients and identified clinical signs and dynamics. By 1954, the care and treatment of latent schizophrenia, in the borderline personality, had departed from psychoanalysis in formulation, focus, technique and paradigm, and had drawn both adherents, and critics adjuring a return to the fold. A full examination of these anomalous qualities will be explicated with the anomalies of schizophrenia (in the next section) as they appear virtually identical, except that the aim of therapy with psychotics is to return the individual to a secondary

process state and with a borderline, not to lose that secondary process state in the first place.

Anomalies that Arose from the Treatment of Schizophrenia

It is interesting to have brought to one's attention the feeling by some family therapists, that most of the pioneers in family therapy,

came from the field of intensive individual psychotherapy with schizophrenic patients; they found, while treating these severe ego disturbances, that they were treating the patients in a vacuum. Treatment progressed only up to the point where insight had to be translated into lasting behavioral changes, then the whole endeavor would collapse, primarily because of regressive unconscious collusion between the family and the patient. These invisible but powerful outside influences, it was reasoned, could only become palpable and manageable if they were integrated into the treatment program. (Framo and Boszormenyi-Nagy, 1965, pp. XV-XVI)

Carl Whitaker, an early family therapist, spoke more autobiographically of this process:

That boy from Harvard that I agonized with for three years didn't get better. He just got quieter. The boy from Menninger's got much better in the three years I worked with him, but he was thrown back into a full-blown psychosis when his parents lured him back home by that new red Chevy convertible. He did keep on calling every six months to tell us how well he was and how things were going so nicely with Mother and Dad, except that between times he would be in that distant hospital again. (1972, pp. 98-99)

The history of the treatment of schizophrenia has been replete with failure; that was one of the reasons, we can infer, that psychoanalysis eventually addressed itself to the disorder(s). As the most successful form of clinical treatment available, it would only have made sense

for the framework to be used, Freud's 1914 paper on narcissism and psychoses notwithstanding. In fact, Freud's 1914 interdiction can be seen as an attempt to protect psychoanalysis from the vicissitudes of therapy with patients of insufficiently stable ego--an extension of the framework that had already occurred! Federn (1943, p. 3) mentions that "Bleuler himself was the first to state that Burgholzli could discharge three times more cases since all physicians had begun to deal with them on the more profound basis of Freudian understanding."

While sympathetic to the future treatment of psychoses, Freud used the term, "some other plan better suited for that purpose" suggesting how closely tied psychoanalytic treatment and a reliable ego were in Freud's thinking (Stone, 1954, p. 567). Stone points out that though Freud was flexible with regard to revision of formulation, he was basically uninterested in experimenting with extensions to new clinical groups or devising new techniques. (p. 567)

Another related problem was the difficulty in communication when dealing with schizophrenia. Verbal production was often unintelligible. Fromm-Reichmann in 1948, addressed this (as well as Freud's 1914 concerns about narcissism), with the implication, it appears, that psychoanalysis was not helpful in this matter.

There seemed to be no medium in which the disturbed schizophrenic and the psychiatrist could communicate with one another. The thought processes, feelings, communications, and other manifestations of the disturbed schizophrenic seemed nonsensical and without meaning as to origin, dynamics, and actual controls. (p. 263)

The use of terms such as "word salad" for the speech of schizophrenia, and the reliance on symbolism may be taken as indications of people's difficulty in communicating with schizophrenic patients. This difficulty could not have made the prospects for a basically verbal form of treatment any brighter. Nevertheless, the framework was extended.

By 1919, Kempf publically addressed himself to the treatment of schizophrenia using modified psychoanalytic methods, with "hygienic measures, vigorous, playful exercises, and...handicrafts" (p. 58) as valuable adjuncts. It was his experience that the treatment was successful, given certain conditions.

The psychoanalytic treatment of repressed, perniciously regressive, dissociated personalities produces astonishingly reconstructive results when an altruistic transference can be maintained and the wish for insight is spontaneous, that is, comes from the patient. This requires upon the part of the physician, sincerity, insight, technical skill, self control and the capacity to win confidence and control the transfer. (Kempf, 1919, p. 58; his emphasis)

Particularly interesting is Kempf's translation of vivid family dynamics into psychoanalytic formulations; he appears to have been fully appreciative of the role of what he referred to as "this family disaster" in the patient's decompensation citing the unconscious "repressive influence of the individual's intimate associates" (p. 58; his emphasis) as causing the "maladaptation of every functional psychosis or neurosis." At another point, he draws a vivid picture of one patient's predicament.

The two families conflicted right and left about the way to raise their only grandchild, and the timid, inexperienced young mother was swept off her feet. Her husband's mother insisted upon plenty of fresh air for the infant and her own

mother protested that they were freezing it. When her husband happened to be in a nearby city his mother insisted that she neglected him because she did not visit him, and her mother objected to the visit because she would be neglecting the baby. (1919, p. 31)

Taken in the context of a husband who was necessarily away from the home for large amounts of time and Kempf's finding after "careful study" that the patient had not one single adult who felt an encouraging sympathy for her efforts, the situation he described is a classic double-bind, with all the necessary formal characteristics, the D-B group elucidated in 1956! He went on to say that the treatment had been successful, and in fact, she was able to leave the hospital with psychosis in remission, except that subsequent family pressures and restrictions undermined his efforts somewhat.

The treatment of schizophrenia by psychoanalysis, however, really began during the 1930's, and accelerated during the 1940's (Jackson and Satir, 1961, p. 34). Zilboorg (1931, p. 508) contended that schizophrenia had no "specific event, no one definite pathogenic factor responsible," and that psychoanalysis also, until just previous to 1931, had been looking for the special aetiology or agent in the pathogenesis of schizophrenia. He argued rather, that schizophrenias were "gradual outgrowths of a series of reactions which at first are not entirely schizophrenic in nature...one can almost invariably find a succession of mental reactions" (pp. 500-501), usually neurotic in nature, until overwhelmed or abandoned.

Zilboorg mentions technical modifications, but they are rather

slight, amounting to increased caution in his choice of words and increased emphasis on reality testing; the technique of free association predominated, however, as did the classical analytic change processes once the patient began to resemble a neurotic dynamically (after approximately one year of preliminary work).

The first period of her analysis (about a year) was devoted to the testing of reality. That is to say, the patient's associations, dreams, memories, or any other statements were carefully analyzed from the standpoint of what was actual or not. The analyst was quite passive; at no time during the whole analysis was any technical language used and at no time was the analytical theory explained to the patient. It must be borne in mind that the use of technical language, or the imparting of any theoretical premises to a schizophrenic patient is rather dangerous and absolutely useless. It is useless because the schizophrenic is a master of cold abstract thinking and the more theory you impart to a schizophrenic, the more abstract material you furnish him for his unreal system of thought. (1931, p. 502) [his emphasis]

Moderate technical modifications due to un-repressed material. In 1934, Federn devoted a three-part series (now regarded as a classic) to the psychoanalysis of psychoses in which he introduced some problems relating to families of patients and some technical modifications. He dated his interest in latent and manifest psychoses from his first analytic case, which Freud had referred to him and for which he served as consultant; unfortunately, the patient developed a psychosis under the rigors of classical analysis. In three such unsuccessful cases, Federn states that "psychoanalysis of the neuroses was the leading cause. In all cases which I later treated with good results, I followed the rules dictated by the libidinous condition of the psychoses, and not those

dictated by the claim for analytical thoroughness." (1943a, pp. 16-17)

He admitted that in some "milder" cases, it had proved possible to treat the neurosis without precipitating a psychosis, but that was a "pseudopschoanalysis which has abandoned the strict Freudian rules..." (1943a, p. 7) which he referred to as a scientifically bad method that achieved good results; his goal was to base good results on a foundation of sound theory. He theorized that the metapsychology of psychoses was based upon,

- (1) abnormal narcissistic cathexis, diminished object cathexis;
- (2) ego regression through which (3) onto-and biologically repressed mental elements and aggregates have become conscious;
- and (4) through which, because of change and diminution in ego-cathexis, the reality test becomes insufficient. (Federn, 1943, p. 3)

This formulation necessitated a number of technical modifications, primarily with respect to case management and transference. Federn included the family of the psychotic patient as the largest factor in case management, stating that

It is not at all astonishing that most psychotics relapse at home or elsewhere when left without the continuous support of transference. Every psychosis is consciously or unconsciously focusing on conflicts or frustrations in family life. Unless these conditions are changed, the cure of psychotics turns out to have been Sisyphean labor which ends in hospitalization or foster-family life. (Federn, 1943, p. 5) [emphasis added]

He noted that because the course of schizophrenia was interrupted by relapses, he refrained from recognizing successes and publishing accounts of his cases until five years after termination of treatment (p. 9). He often treated patients while they remained in the home and took care "not to arouse fear and violence between the patient and the family"

(1943b, p. 248) and felt that no patient could be cured unless the family wished it and that prognosis was particularly poor in the face of a family's hatred, whether conscious or unconscious (1943a, p. 17).

Federn characterized the transference of psychotic patients in the same terms as Bychowski, adding that the ambivalence is more extreme (1943a, p. 252) and if not contained by reality-invoking procedures, could lead to either deification of, or aggression against, the analyst (1943b, p. 247). To this end, Federn eschewed both the couch and the recumbent stance (1943b, p. 247), allowed phone calls and extra appointments (1943b, p. 254), and curtailed free association (1943b, p. 246). Because of the power of the negative transference, the maintenance of the positive transference was important, as were the relational elements of the treatment. Though Federn did not explicitly deal in relational dynamics, his approach to the patient did (1943b, p. 251).

One wins the normal transference of the psychotic by sincerity, kindness, and understanding. It is a great error to believe that whenever a psychotic feels that you understand him he is yours. Frequently he offers opposition at first, but often by the next day the explanation has been accepted. One must avoid blame and severe admonition, any smiling superiority, and especially any lie. There are no white lies allowed with psychotics. To lie to a psychotic is contrary to the injunction in the Bible that one must not place a stone in the way of the blind. To be slapped in a friendly way on cheeks, shoulder or buttocks, is to be treated like a silly child, is an indignity. (Federn, 1943b, p. 251)

The final requisite to successful treatment Federn discussed was a radical change in traditional case management--he used a "motherly helper" outside of the analytic hours.

While every neurotic patient easily transfers from his mother to the psychoanalyst, the psychotic does not do so to a male analyst. This demonstrates how the psychotic depends more on reality than the neurotic, i.e., when he is forced to transfer his mother-relationship to a man, he confuses homo- and heterosexual feelings and becomes more perturbed.

The writer's contention that there should be women helpers for psychotic persons is therefore well founded, although the conclusion was reached as a result of simple experience. In all cases in which the writer was successful, he had such motherly aid; in some cases, the real mother was willing to help, because many women, although lacking in sublimated instinctual motherhood, have a great sense of duty toward a poor psychotic child. But the real mother is usually less helpful than a sister or a nurse who becomes a sister. The relation of a psychotic becomes too possessive and regresses easily to incest, when nursed by his own mother. Yet the loving cooperation of the mother is very helpful, when obtainable. (Federn, 1943, p. 254)

He felt that no psychoanalysis of psychotics could be accomplished without this assistance. The helper would generally be called into the final minutes of each analytic session while the analyst, with the patient's assistance, repeated the problems and solutions dealt with in the session (1943c, p. 480). The helper was invaluable if positive transference were lost, as treatment could continue through her (1943b, p. 256); also the patient received assistance and protection between the analytic hours (1943a, p. 5). He gives an account where apparently his own wife and family were used in this manner (1943a, p. 8).

This clearly constitutes a radical departure from psychoanalytic technique and his case management involving the family, though less startling, is similarly a departure. The emphasis on increasing the resistances to psychotic processes and increasing the influence of the

ego processes stood traditional psychoanalytic processes on their head. And the implicit emphasis on relational aspects of treatment directly counter both the technical and theoretical aspects of classical psychoanalytic treatment.

He added, in a summary-type statement:

The technical innovation does not contradict Freud's teaching, for he developed his method for the treatment of neurosis and not of psychosis. Freud repeatedly said that psychotics were not suitable for psychoanalytic therapy. Today his thesis still holds true when one wants to use the standard method; however, it is no longer true when one wants to know how to modify it. One should not assume that the modified method is easier and less strict. As Freud said, "one cannot make a reliable contract with the psychotic ego." Therefore, it is only with the greatest precaution that we use a method which brings more psychotic material to the surface. (Federn, 1947, p. 139)

This sort of statement leaves Federn in a bit of difficulty. He essentially has put himself in a position of saying that Freud was correct regarding the unsuitability of psychotics for analysis, but only for the classical method. A modified method is appropriate, but no longer classical psychoanalysis; however, the modified method involves a contradiction if both were called psychoanalysis. One uses a method intended to bring unconscious material to the surface to get just the opposite effect, that is, to return conscious unconscious material to a repressed state. Psychoanalysis was designed explicitly to make the unconscious conscious, for use with latent schizophrenics and psychotics; however, psychoanalysis was modified explicitly to make the conscious unconscious. The position is not an enviable one.

Family homeostatic anomalies with schizophrenia. During, and immediately subsequent to, the re-formulation in theory and technique that allowed a significantly different "psychoanalysis" success with psychotics, analysts were also drawing the discipline's attention to a rather odd set of unexpected and unpredicted phenomena related to schizophrenic patients. It gradually became apparent that family processes contemporaneous with the course of schizophrenia could improve or worsen the patient's condition, and concomitantly that changes in the schizophrenic's condition at times affected the condition of family members.

Kasanin and Knight (1934, p. 262) pointed out compensatory behavior on a father's part to a mother's over-protection of a child, who in turn, looked for and encouraged this degree of protection. In another case, despite obvious wishes on the part of parents for the recovery of the schizophrenic adult child, Kasanin and Knight stated that,

It is curious to note that the patients with over-protective parents remain in the hospital for only short periods of time because the parents invariably make every attempt to remove them from the hospital, irrespective of their mental condition, and make every effort to bring the patients back to their old environment even though they do not fit there.
(1934, p. 257)

For the parents to remove the schizophrenic family member from hospitalization with this degree of regularity, more than coincidence or idiosyncratic family patterns are operative; one can infer that it is in some way important for the schizophrenic member to be brought home.

Cohen and Lipton (1950) reported on three cases of acute schizophrenic psychoses that underwent remission shortly after a maternal

death. They stated that while this was a familiar phenomenon to clinical observers, their literature search failed to reveal any reports on the subject. Actually, that is not very surprising, as most schizophrenic patients were treated in a hospital setting and, through a combination of factors, within a psychoanalytic model, which avoided by and large contact with family members. The occurrence of these spontaneous remissions was probably often overlooked. Also, psychoanalytic theory could not easily account for this type of contemporaneous occurrence, and those remissions recognized as such were probably shrugged away as coincidental, or not reported as they fit into no recognized framework and did not fit into the pre-eminent psychoanalytic approach. It is a safe bet that most of the people in a position to write such reports were the least likely to "see" the remission, either practically or through the framework filter.

Cohen and Lipton's study was initiated

by the coincidental occurrence of the phenomenon in two male patients within a period of a few months on the insulin service of Brooklyn (New York) State Hospital. Both patients showed striking remissions of psychotic behavior within a short time after they were informed of the deaths of their mothers. A third case reported here was observed more than a year before this study was started, but is included because it illustrates the same phenomenon in a female patient. (1950, p. 716)

Apparently, the multiple occurrences within a short time period increased the salience and validity of the phenomenon. They reported similarities in the three patients: all three were young adults with recent onset of first psychosis; in all three cases, mothers were actively involved in

the delusional and/or hallucinatory material and all reacted to the deaths with some degree of guilt (p. 723).

In 1954, Ackerman explicated some of these homeostatic shifts:

It is by no means rare in the treatment of a family pair that as one member of the pair gets better, the other gets worse. In child guidance work, as the child improves, not infrequently the mother paradoxically worsens. Or, as the child responds to psychotherapy, the parental conflict becomes drastically intensified. Similarly, in the treatment of marital problems, it is often the case that as one marital partner matures and becomes sexually more adequate, the other regresses; or one may respond to analytic therapy with an increased capacity for closeness, and the other may react with depression. (p. 362)

Moreover, Ackerman's experiences led him to report the presence of paradoxical shifts in interpersonal relationships that defied individual formulation. For example,

...a wife campaigns for her husband to enter psychotherapy for sexual impotence, threatening to leave him unless he is cured. The husband yields, is treated, and the symptom of impotence is quickly alleviated. The husband's therapist, pleased with his success, is shocked to discover that directly after the husband's potency was restored, his wife deserted him. This is paradoxical behavior, to be sure, but it can and does occur. Individual psychotherapy may help the individual, but under certain conditions it may fail to ameliorate the psychology of a family relationship. The tension of interpersonal conflict may remain largely unabated even though intrapsychic disturbance is measurably relieved.

Similarly, in 1958,¹⁴ Fisher and Mendell reported that significant changes in the identified patient were accompanied by clear cut changes

¹⁴This study was published in 1958, two years after the initial double-bind publication but is included here as an indication of the pattern of the times. Fisher and Mendell cite none of the double-bind

in other members of their families; this occurred in all ten of the families they investigated (p. 134).

The issue of shifting interpersonal patterns and reciprocities in families with a schizophrenic member became so important, and frequently substantiated that by 1964, an article had appeared which explicitly asked, "Can a family which has a schizophrenic adolescent member have other offspring who are emotionally well-adjusted?" (Friedman, 1964, p. 47) Not unexpectedly, the author's answer was "no."

Development of the Relational Aspect of Treatment for Schizophrenia

Parallel to the elaboration of psychoanalysis and the emergence of the psychoanalytic anomalies, a psychiatrist, Harry Stack Sullivan, developed a treatment approach for schizophrenia based upon different paradigms and assumptions. This alternative DM, unlike psychoanalysis, was developed specifically with respect to schizophrenia and related syndromes, i.e., certain types of obsessionalism and paranoia. By the late 1930's and early 1940's the Sullivanian, or interpersonal, approach had gained a moderate, but influential, number of adherents among psychoanalysts. Though not an analyst himself, and despite the fact

literature nor any of its antecedents and appear to be reporting from within a psychoanalytic framework as they mention similarities in fantasies and defenses, as well as in the therapeutic effects. Also, if this was the second such study they had conducted, and this one reported on ten families, one can assume the project had been some time in the making.

that Sullivan's approach was not at all psychoanalytic,¹⁵ a relatively large number of analysts working with schizophrenics, found it helpful. A degree of controversy arose in the late 1940's and 1950's regarding the advisability of treating the psychoses, particularly schizophrenia, and the differences in technique and conceptualization between the classical or interpretationist psychoanalysts and the interpersonal or relational psychoanalysts. Because these events will be seen to be important in the inception of the DB paradigm and in understanding the felt crisis of that time, Sullivan's DM will be very briefly explicated, then two representative articles from relational psychoanalysts will be used for illustration and as examples of the relational approach of that time.

Harry Stack Sullivan's interpersonal psychiatry. After obtaining his medical degree at the Chicago College of Medicine and Surgery in 1917 and his discharge from military service after World War I, Sullivan was sent in 1922 to St. Elizabeth's Hospital in Washington, D.C. as the Veteran's Bureau liaison officer for a year (Mullahy, 1970, p. 2). There he met Dr. William Alanson White, whose flexible approach

¹⁵ Mullahy (1970, p. 7) remarks that early in his career, Sullivan found Freud's work helpful, but as time passed and as Sullivan increasingly concentrated on schizophrenia, he used Freud less frequently until by the middle 1930's, Sullivan had fairly well developed his interpersonal practice and theory. Though Sullivan's first two articles (1924-25; 1925) were formulated with the classical Freudian DM, Sullivan's own DM is not psychoanalytic in paradigm, theory, or practice.

became an important influence on Sullivan's clinical practice. Sullivan then went, in 1923, to the Shepard and Enoch Pratt Hospital in Baltimore where, two years later he became Director of Clinical Research. It was during his five years at Shepard and Enoch Pratt that Sullivan became convinced of the great importance of social (in the sense of interpersonal) factors for the etiology and treatment of psychopathology, particularly schizophrenia. During this five years, Sullivan's papers became progressively more outspoken in approach. During approximately his last year at Shepard and Enoch Pratt, Sullivan established a unique receiving unit to treat schizophrenic men (Mullahy, 1970, p. 2)¹⁶. This unit was set up according to Sullivan's conception of a social-psychological unit (what is now commonly referred to as a psychotherapeutic milieu); treatment included the effects of such a social-psychological setting, including contact with attendants trained by Sullivan, as well as the traditional hour-long sessions with the therapist (Mullahy, 1970, p. 4). This unit, (during about 1928), was an expression of his formulations regarding the interpersonal etiology and treatment of schizophrenia.

What can be considered Sullivan's schematic, but well-developed paradigmatic work appeared in 1931. In this work, Sullivan outlined

¹⁶ Sullivan eventually treated only men; his position was that schizophrenics were difficult enough to understand, he was not going to attempt to add to this the vicissitudes of understanding another gender, with what the gender differences implied with respect to schizophrenia.

his method of treatment in the unit he had established, and his formulation regarding etiology and mechanisms of change for schizophrenia.¹⁷

As will be seen, both etiology and treatment of schizophrenia are conceptualized as interpersonal rather than intra-psychic.

The procedure of treatment began with removing the patient from the situation in which he is developing difficulty, to a situation in which he is encouraged to renew efforts of adjustment with others...The non professional personnel with whom the patient is in contact must be aware of the principal difficulty--viz, the extreme sensitivity underlying whatever camouflage the patient may use. They must be activated by a well-integrated purpose of helping in the re-development or development de novu of self-esteem as an individual attractive to others. They must possess sufficient insight into their own personality organization to be able to avoid masked or unconscious sadism, jealousies, and morbid expectation of results...

Given the therapeutic environment the first stage of therapy...takes the form of providing an orienting experience. After the initial fairly searching interview, the patient is introduced to the new situation in a matter-of-fact fashion, with emphasis on the personal elements...He is made to feel that he is now one of a group composed partly of sick persons - the other patients - and partly of well folks - the physician and all the others concerned. Emphasis is laid on the fact that something is wrong with the

¹⁷ Sullivan, H.S. The modified psychoanalytic treatment of schizophrenia. Am. J. of Psychiat., 1931, 11, 519-536. Sullivan's paradigmatic statement underwent revision, of course, but the timing of these revisions is difficult to ascertain. Sullivan apparently had a horror of being misunderstood, and so preferred to lecture where he could correct misunderstandings. Most of his later work was in seminar format at Chestnut Lodge which was tape recorded and published posthumously by his adherents and colleagues (Mullahy, 1970, p. 6). While lines of development in Sullivan's thinking are discernible, their timing is usually not. This 1931 article, being a relatively early work, was published soon after formulation and therefore is relatively fixed in time.

with the patient and...that regardless of the patient's occasional or habitual surmise to the contrary, everyone who is well enough to be a help will...be occupied in giving him a chance to get well. From the start he is treated as a person among persons (Sullivan, 1931; reprinted in Mullahy, 1970, p. 273; Sullivan's emphases)

Early efforts were directed toward establishing precipitating factors for the psychosis, and reconstructing a chronology that included events, experience, and the behavior of people close to the patient. Efforts were made to point out that however mysterious the psychotic manifestation that had befallen the patient, they were related to his everyday living among a relatively small number of people important to the patient (Mullahy, 1970, p. 28).

To these ends, Sullivan eschewed free association and interpretation (1970, p. 28), preferring a form of guided dialogue, with the therapist in the role of a participant-observer (1970, p. 41). The psychotic communication of the patient was regarded as informational, particularly with respect to maintaining some distance from people and also in maintaining some degree of "personal security" (the condition when anxiety was relatively low). Schizophrenic speech was neither regarded as "word salad" (i.e., meaningless and random), now was it interpreted symbolically as in psychoanalysis. Procedurally this was expressed as a relative devaluation of verbal production and an emphasis on the reality relationship between therapist and patient developed during treatment. The mechanisms of change occurred during the development of the reality relationship, which was conceptualized as both

conceptual and curative. Through the relationship with the therapist, the patient emotionally corrected problems in his injured or faulty narcissism.

The emotional re-learning or re-working of the narcissistic deficits obviously has some relationship to Freud's paradigmatic working through the repetitions in the transference relationship. The differences are that in the relational approach the locus of difficulty is regarded as concerning self-esteem rather than conflict; the relationship is a reality relationship rather than transference; moreover, the technique of dialogue rather than free association and interpretation is directed toward the integration of psychotic events and precipitating factors of significant others rather than the integration of conscious and unconscious elements.

Concomitantly, schizophrenia as conceptualized by Sullivan, was not so much the welling-up of id forces, but was rather attributable to processes wherein the individual had never been able to build up sufficient self-esteem; when increased stress occurred, (usually developmental), the already impaired self-esteem crumbled altogether. The person was conceptualized as having been subjected very early in life to anxiety-provoking experiences which undermined his sense of fundamental personal security (Mullahy, 1970, p. 10).¹³ "Cultural

¹³In Sullivan's more mature formulations, anxiety grew in importance and was characterized as a "felt threat to, or actual loss of, self-esteem owing to the actual anticipated, or imaginary disapproval

distortions" learned in the home were of prime importance in later developing schizophrenia; these were regarded as erroneous attitudes and unfortunate occurrences in the family. The early experiences with anxiety combined with the

various other unfortunate experiences, maldevelopments, and distortions that the individual suffers may all combine to render him abnormally vulnerable to the demands and stresses of the acknowledged stage in Western society...(Mullahy, 1970, p. 484)

Sullivan felt that though the complex etiology of schizophrenia culminated in a situation in which the sexual adequacy of the individual (according to that individual's learned standards) was found "acutely unsatisfactory," the cultural distortions learned very early in the home were of primary etiological importance (Mullahy, 1970, p. 19)

As such, Sullivan rejected the idea that unsuccessful resolution of the Oedipus complex was of etiological significance and in fact regarded the complex, not as a universal, nor biologically based occurrence, but rather, the result of "multiple vicious features of our domestic culture." (Sullivan, 1926; cited in Mullahy, 1970, p. 14) Moreover, not only was etiology based on early family experiences, recovery in large depended upon the social milieu to which the patient returned (Mullahy, 1970, p. 18)

of significant other people, or of disapproval of one's self, owing to the values and ideals one has acquired or developed." (Mullahy, 1970, p. 484). This anxiety is transmitted by the mothering one to the infant and may be derived from some action of the infant of which the mothering one disapproves, or from some concomitant anxiety induced in the mothering one from elsewhere.

Interpersonal psychoanalysts. By the later 1940's and early 1950's, there were those analysts who retained the classical interpretationist approach and maintained that the psychoses were not the province of psychoanalysis, those who allowed that technical modifications had allowed psychoanalysis to be successful with these disorders, and yet another set who proceeded along quite different lines, adopting the Sullivanian interpersonal paradigm and DM for their work with schizophrenics. This latter group continued to refer to themselves as psychoanalysts, and they had, in fact, received psychoanalytic training, yet they used a Sullivanian rather than Freudian DM.

Fromm-Reichmann's writings during the 1940's illustrate this latter group. She constitutes an example of an analyst who moved from traditional psychoanalytic formulations to the changes in what she termed "the doctor-patient-relationship and the approach to the contents of psychotic communication" (1948, p. 265). She indicated that the results of psychoanalytic therapy with schizophrenics had been, thus far, "not too discouraging," but that cures had not been to psychoanalysts' satisfaction with respect to frequency, or durability (1948, p. 272).

In Fromm-Reichmann's terms, the changes in approach included a relative devaluation of the verbal productions of schizophrenic patients and an emphasis on the reality relationship developed between patient and therapist during treatment. In technique, the emphasis on symbolism in schizophrenic speech was suspended, and the "overemphasis of contents was discarded." (p. 269)

Technically, the patient's confusing speech patterns were no longer interpreted symbolically; rather therapeutic attention was focused on the genesis and dynamics which determined the contents of what was said, particularly as they applied to the schizophrenic episode.

As a way of accomplishing this, close attention is paid to, and careful investigation done about the following: present timing and circumstances, the original setting, precipitating factors and bodily and emotional symptoms preceding or concomitant to a psychotic manifestation. The patient is trained if he is in contact to join the psychoanalyst in his endeavor to find those connections. We have been gratified by the disappearance of psychotic manifestations subsequent to their consistent, repetitive, generic, and dynamic scrutiny...this procedure leads automatically toward the investigation and understanding of neighboring symptomatology which has been linked up with the manifestations originally under scrutiny. (1948, p. 269)

This was not tantamount to free association, which was regarded as a mistake with schizophrenics in that it loosened up thinking that was already quite disorganized (p. 270); the form of the interaction was Sullivanian, emphasizing dialogue, precipitants, and experience, in relation to the psychotic episode.

Technically, this collaboration was possible only after patient and analyst had formed a workable doctor-patient relationship that had established a consensus about the need for treatment and its reasons (p. 267). Significantly, in two other extensions of psychoanalysis to new clinical populations, the therapists explicitly set about establishing a working relationship, i.e., consensus between patient and therapist regarding the need and reasons for treatment. Anna Freud

devoted chapters of The Psychoanalytic Study of the Child to the need for establishing the working relationship with the child patient, and the importance of the needs and reasons for treatment (which is particularly important with children as it may be their parents, and not themselves, who are suffering from some of the child's symptoms). Similarly, Jacobson (1954) in a discussion about treating psychotic depressions, stated that the quality in the analyst's responses were more important than the quantity of sessions, (p. 603) and emphasized the genuineness of the analyst's response and demeanor toward the patient (p. 604).

In any case, what those patients need is not so much frequency and length of sessions as a sufficient amount of spontaneity and flexible adjustment to their mood level, of warm understanding and especially of unwavering respect; attitudes which must not be confused with over-kindness, sympathy, reassurance, etc. In periods of threatening narcissistic withdrawal, we may have to show a very active interest and participation in their daily activities and especially their sublimations. I have observed that analysts who are rather detached by nature seem to have difficulties in the treatment of depressives. Beyond this warm, flexible emotional atmosphere, without which these patients cannot work, supportive counter-attitudes and interventions may occasionally be necessary; but they are only a lesser evil for which we have to pay. (Jacobson, 1954, p. 604)

Fromm-Reichmann's work, based as it was on Sullivan's, explicitly emphasized the importance of the relational element in which the therapist was the participant-observer in the interaction between him/her and patient. The existence of this reality relationship of course, did not preclude the development of transference, but Fromm-Reichmann took issue with Abraham and Federn that the analyst should refrain in

analyzing it. The investigation of the doctor-patient relationship and its distortions were construed as essential to the therapeutic process; besides the distortion, however, the existence of a "real, positive interrelatedness" between the patient and therapist was recognized, and was included in discussion (p. 267).

An important ramification of such an approach relates to the counter-transference of the therapist and the elements of the reality relationship that are initiated by the therapist. Reactions of rejection, or postures of grandeur were clearly problems in this sort of enterprise and the personality of the therapist came to be regarded as more important.

Similarly, in 1952, Powdermaker reported on observations from treating schizophrenic patients using the work of Sullivan and Fromm-Reichmann as a guide; Powdermaker regarded their central thesis about treatment to have been an emphasis on the relationship between patient and therapist, through which the patient (and often the therapist) was brought to an understanding and acceptance of the realities of that situation and eventually other situations (1952, p. 62, footnote #3). In her treatment of schizophrenics, Powdermaker emphasized the reality relational components and downplayed the classical analytic techniques.

During therapy, it did not seem advisable to have the patient use the couch since this encourages reverie and retreat from reality. And as far as possible I tried to think about the patient's communications without being influenced by preconceived ideas of interpretation. So it seemed indicated to have these schizophrenic patients see and relate to me as a real person, a procedure unlike

the conventional analytic treatment of neurotics. Phantasies were not encouraged. The manifest content of dreams was usually readily interpreted by the patient; he was not necessarily encouraged to associate to find the latent content, though he often brought it out. In general, problems of the present and those just preceding the breakdown were considered together with the conflicting attitudes and feelings involved in them...This procedure of course often could not be carried out in this order but was the pattern of therapy which I tried to keep in mind. (Powdermaker, 1952, pp. 62-63)

From Powdermaker's description of the treatment plan, it is obvious she followed the Sullivanian and Fromm-Reichmann pattern closely.

What is particularly interesting is the timing of her report, only four years before the D-B report, and the many ways in which her observations addressed interpersonal elements later explicated and formulated by the D-B hypothesis and theory.

For instance, Powdermaker considered the "dilemma" of the schizophrenic to lie between the need to communicate, and the fear of doing so.

The dilemma of the schizophrenic presented itself in my observations at the hospital in a thousand ways---but always it was the same dilemma. It is as if the schizophrenic were saying: 'I want to communicate. I'll do it but I'm afraid to, so I'll say it so you can't understand it or I'll pretend not to know you are there.' The schizophrenic calls attention to himself by negativism, flirting, stereotyped gestures, all of which dare you to communicate with him, to accept and understand him; and then he retreats... His need to relate seems to be second only to his fear of it. (1952, p. 61)¹⁹

¹⁹The D-B people would add a third disqualifier perhaps, "...or I'll pretend it's not me saying this and such." The point is, Powdermaker had noted the ambivalence and disqualification as central to schizophrenic complexity of speech, rather than having emphasized its symbolic content.

Similarly, she was aware of the schizophrenic's perplexity, and response, as though nothing made sense.

It is the inability to hold their own with authority figures who seem illogical or otherwise ununderstandable that has made life so difficult for the patients in the first place. When the doctor also tries to put over his own ideas, the patient's resistance may become insurmountable. The need of the patient for someone who will try to understand what he is endeavoring to communicate is made clear by the remark of a patient: "If the others don't agree, it means you're wrong--it takes so much strength to be a minority of one." (p. 63)

The D-B people later demonstrated that indeed, nothing much did make sense in the families of the schizophrenics they were observing.

What is perceived by the schizophrenic patient is the battle to maintain a self and the need to relate to others at the expense of compliance. In persons who have experienced some acceptance of the self and have therefore been able to develop some degree of self-esteem, there can develop some degree of corresponding ability to sustain non-conformity with those around one (1952, p. 70); however, experience had led these patients to believe that if they expressed their perceptions and reactions, they would not be accepted (p. 67). This in fact was shown to be the case, and the perceived battle between maintenance of a self at the cost of maintenance of relatedness appears to have been also accurate. The active ignoring by mothering ones of the feelings and perceptions of young schizophrenic adults was noted, as well as the (not surprising) tendency of the patients to hide their feelings and perceptions. This set of observations went far in explaining one

of the frequent and "seemingly paradoxical statements made about the schizophrenic: that he is 'often so intuitively understanding of people and yet he is seemingly unable to take in and react to the reality around him'." (p. 67) The D-B people provided the rest of the explanation for this odd immobility in the face of perceptiveness.

Finally, Powdermaker concluded that schizophrenics used fundamentally different defenses than neurotics, for what appeared to be some of the same problems, and that the schizophrenic defenses were "related to a way of dealing with relationships involving perceptions, and the feelings and ideas about them;..." (p. 69) As will be seen, that perception about perception would be borne out.

Implications for Classical Psychoanalysis of the "Non-traditional" Anomalies

Anomaly characteristics and effects on psychoanalytic therapy. This latter set of anomalies proved discrepant to expectations with regard to both technique and theory in a number of respects. Freud had adjured against the psychoanalytic treatment of psychoses on the grounds that the prevailing narcissistic clinical picture prevented the development of transference which would necessarily preclude the treatment mechanisms of change and thus, cure. Psychoanalytic treatment of psychoses did proceed, however, often by default with the latent schizophrenias, and subsequent developments proved Freud wrong in relation to the issue of transference. Psychotics did, in fact, develop trans-

ference of unprecedented intensity, primitiveness, and dependency. The occurrence of transference with these patients, regardless of its characteristics, constitutes an anomaly to theory. According to Freud's position, because of the investment of libido in narcissistic concerns, little or no libido should have been available with which to form transferences; that these transferences were so intense merely compounded the degree of discrepancy between paradigm-induced expectation (and actual prediction in this case) and empirical findings.

Freud's prediction was correct, however, regarding the difficulties in curing narcissistic disorders with psychoanalytic treatment, though not necessarily for some of the reasons he advanced. It was found that thoroughly working through the repetitions in the transferences with all the object libido implications, did not result in cure; in fact, such a process more than occasionally exacerbated the psychoses. This was a crucial anomaly, as it struck at the heart of the psychoanalytic paradigm, at Freud's final formulation of the change process in psychotherapy. According to that final formulation, which had proved unprecedentedly successful with neuroses, the affectively laden working-through of repetition compulsions within the context of transference constituted the effective change process; decades of success in analysis of neurotics had demonstrated its efficacy. Yet in work with the psychoses, it proved ineffectual. This was particularly damaging as it proved anomalous to the paradigm itself, and not merely to a theory or DM elaboration.

This turn of events led some analysts to begin focusing on the narcissistic core of the disturbances. They began to propose a new explanatory complex--that the cause of psychoses, had to do, not with external object libido, as in neuroses, but, with narcissistic libido processes and that this necessitated a shift in the focus and processes of therapy from object libido concerns to narcissism, which could be treated. Thus two more theoretical points were changed to suit empirical circumstances. The consequences for treatment reflected directly upon Freud's paradigm regarding change. Therapy of the narcissistic core consisted of re-integration of the split in ego formulation through a re-educative emotional process that was not achieved through the release of object libido; the ego split, moreover, was not between pleasure (id processes) and reality (ego functions), but between an archaic unmodified, and primitive narcissistic core, and its overlay of learning and development. The focus of change was on integrating the unmodified narcissistic core with its development overlay rather than on the compromise of pleasure and civilization. With neuroses, releasing the libido processes made them available for work; with the psychoses, they were already released from this undeveloped core, and the other, later developments of the ego were awash in these libido processes; the operative change process consisted in minimizing their presence and working with the injured, but meglomaniacal, narcissism.

The shift to narcissism as cause and focus of change efforts had a variety of technical consequences. The defense of psychoses were

obviously seen as quite different from those of the neuroses. The analyst's demeanor and behavior towards patients with narcissistic disturbances of necessity, had to be different than towards neurotic patients. If the depriving, frustrating processes were instituted, the transference reactions would be overwhelming--in part, because of the difficulty of these patients in controlling affective intensity, especially rage, but particularly because this form of frustration complemented and elicited the affective structure that constituted the disorder: ambivalence, rage, depressions almost anaclitic in depth, and avoidance of personal contact.

For these reasons, intentionally frustrating elements of therapy were eliminated or diminished as far as possible within reality constraints of a therapeutic situation. Similarly, the elicitation of primary process material was stopped; therefore, technically, free association was dropped with these patients and a dialogic quality emerged. Concomitantly, the role and functions of interpretation became markedly less important--there grew to be less opportunity for it in dialogue than in free association, and the raison d'être of interpretation (to make the unconscious conscious) was not wanted for these patients. Similarly, "reality markers" were no longer eliminated; that is, those elements of psychoanalysis that had been designed to diminish reality testing and increase development of transference were definitely not wanted. Therefore, transference was not allowed to develop too far without reality correction, analysts eschewed the use of the

couch and also allowed patients to face them (or some version thereof; Sullivan preferred, for instance, to have himself and the patient face the same point through sitting 90° from each other so their paths of vision cut across each other's. That allowed him to see the patient's non-verbal reactions without having to stare at the person for an hour at a time, or have the person stare at him -- an important consideration for Sullivan).

With the decrease in importance of transference, free association and interpretation, there developed a greater appreciation of the reality aspects of the patient-therapist relationship, and an emphasis on the reality relationship as both contextual and curative. As such, the change process in psychotherapy shifted from verbal to relational; that is, from interpretation of linking conscious statement to unconscious content, to relational re-education of fundamental narcissistic concerns. It is for this reason, one can infer, that Sullivan and his adherents concern themselves repeatedly with the necessity of the therapist's respect for the patient and the necessity of not appearing the incomparable paragon of health and holiness. The self-esteem of the psychotic was already at issue; to appear disrespectful on any level, or perfect, would serve to push the patient further down into the already injured lack of self-esteem. In fact, Sullivan and adherents differed from psychoanalysis in attributing psychosis, not to the welling up of libidinal forces through a weak ego, but rather to processes wherein the individual was never able to build up sufficient

self-esteem. At this point, it is clear that the analysts adhering to relational concepts and treating psychoses (both manifest and latent) and the analysts adhering to verbal concepts and treating neuroses, diverged sufficiently to be different DMs with different paradigms and paradigmatic elaborations.

Response in the psychoanalytic communities. The response to the anomalies and technical modifications was heated controversy, with each group further elaborating its respective framework. The ensuing developments (during the late 1940's through the middle 1950's at least) can be seen as centering around two major controversies. First, these new debates about whether family members could be seen as a legitimate aspect of psychoanalytic work; for instance, would seeing the family preclude transference and therefore change? Also, if the family were seen, could this psychotherapy continue to be called psychoanalyses? Secondly, there were debates about the technical modifications, particularly between those analysts who had adopted a Sullivanian relational approach and those who insisted upon a return to the fundamentals of classical practice, i.e., interpretation of transference.

As has just been reviewed, this interpretationist versus relationist split occurred with regard to the treatment of psychosis. It also occurred with respect to child analysis. In Anna Freud's 1946 Psychoanalytic Study of the Child (which includes her 1926 Introduction to the Technique of the Psycho-Analysis of Children (p. XI), she

challenged the traditional conceptualization of the role of transference as an "intermediate step" between neurosis and health, and differentiated treatment of the child from that of the adult in this respect.

The opinions expressed in the Third Lecture, on The Role of Transference in the Analysis of Children, have during the last twenty years been repeatedly opposed by children's analysts in England and America who maintain that the children under their treatment show profuse signs of transference which is open to analysis in the same manner as in the analysis of adult patients. The author fully agreed that this is the case. But, in spite of these manifold and variegated transferred reactions of the child, the author has not, so far, met a single case of a child patient where the original neurosis was given up during the treatment and replaced by a new neurotic formation in which the original objects had disappeared and the analyst taken their place in the patient's emotional life. It is only a structure of this kind which deserves the name of transference neurosis. So far as the author's experience goes, the latter occurs solely in cases of adult neurotics who are treated with the classical technique applicable only to patients who have reached maturity. (A. Freud, 1946, p. xii; emphasis added)

The necessity of special techniques for children was insisted upon, and attributed to the fact that children, unlike most adults, are "immature and not self-respondent." (1946, p.4) Specifically, the child patient lacked "insight into the malady, voluntary decision, and the will towards cure" (1946, p. 5), as the child was often not the sufferer in his/her disorder. For these reasons, there had to be a preliminary phase of treatment in which the

small patient [is made] 'analysable' in the sense of the adult, that is to say inducing an insight into the trouble, imparting confidence in the analyst, and turning the decision for analysis from one taken by others into its own. Children's analysis requires for this task a preparatory period which does not occur with adults. I must

emphasize that everything which we undertake in this period has nothing to do with the real analytical work, that is to say there is as yet no question of making unconscious processes conscious or of analytical influence on the patient. It is simply a matter of converting an unsuitable situation into a desirable one, by all the means which are at the disposal of an adult dealing with a child. This time of preparation--the "dressage" for analysis one might properly call it--will last the longer, the further the original condition of the child is from that of the ideal adult patient which has already been described. (A. Freud, 1946, p. 6)

All of the efforts to ready the child for psychoanalysis were directed toward establishing "a very definite emotional relationship with it. The harder the work to be done, the higher must be the strain-capacity of this attachment." (1946, p. 38). Freud regarded this attachment as independent of analytical theory and technique (p. 38), most definitely as the precursor to treatment, and not treatment itself, and characterized this preparatory work as only a more formal and explicit form of the accommodations most analysts made in the initial period of analyses with any patient (p. 16). Nevertheless, she insisted upon its importance as the context for treatment clearly enough that she drew fire for several years from Melanie Klein and her group (1946, p.5); the Kleinian group disagreed strongly with the necessity of establishing this emotional relationship preparatory to analysis.²⁰

²⁰ As mentioned previously, the Kleinians had also abandoned verbal techniques for play therapy.

In 1954, Freud addressed the relational issue once again, with respect to adult neurotic and psychotic patients, challenged the general conception of transference versus reality relationship, while acknowledging the unpopularity of any form of challenge to the classical ascriptions of transference (1954, pp. 618-619)

Many analysts express the opinion that the patient's transference,...is strongest when he enters the treatment, and has to be worked through gradually through interpretation until, at the very end of treatment, a real relationship can come to the surface. This may be true for psychotic and borderline cases; for the common neurotic case the reverse order seems to me to be the rule. We see the patient enter into analysis with a reality attitude to the analyst; then the transference gains momentum until it reaches its peak in the full-blown transference neurosis which has to be worked off analytically until the figure of the analyst emerges again, reduced to its true status. But--and this seems important to me--so far as the patient has a healthy part of his personality, his real relationship to the analyst is never wholly submerged. With due respect for the necessary strictest handling and interpretation of the transference, I feel still that we should leave room somewhere for the realization that analyst and patient are also two real people, of equal adult status, in a real personal relationship to each other. I wonder whether our--at times complete--neglect of this side of the matter is not responsible for some of the hostile reactions which we get from our patients and which we are apt to ascribe to "true transference" only. But these are technically subversive thoughts and ought to be "handled with care." (A. Freud, 1954, pp. 618-619)

Oberndorf, not known as one of the relational analysts though clearly awake to relational elements, also maintained an emphasis on recalling and working through unconscious material through interpretation of transference and resistances, but added, "I might add also those subtle, unobservable and indefinable phenomena which occur in

the relationship between physician and patient." (1950, p. 394) That this trend toward recognition of relational elements across the disorders has continued is obvious; in 1974, Appel stated "...the difficulties inherent in the classical approach to psychotherapy are apparent...Whatever the orientation, an increasing consensus of technical theoreticians appears to be moving in the direction of a therapeutic role that is reconstructive primarily via relationship rather than interpretation." (pp. 103-104)

The relationists during this period, however, were most definitely not in the majority, and the responses of interpretationists varied, though none encountered during review, approved of the relational shift. Eissler (1953) in particular, advocated a return to strict interpretationist lines, and accorded a "special place to a purely interpretative technique." (p. 126) Eissler regards interpretation as the "exclusive tool of therapy" (p. 109), differentiating it from transference, which though "therapeutically effective," was considered a source of energy which properly used led to recovery, but not a tool of therapy (pp. 108-109). Similarly,

There are other therapeutically effective factors which may look like tools, such as the denial of wish fulfillment, to which the patient must submit through the treatment or, more generally, the psychoanalytic therapeutic attitude. I believe that these factors are secondary; that is to say, they are the necessary consequences when interpretation is the only tool of the analyst. Similarly, working through is a specific technique for using interpretation. (Eissler, 1953, p. 109)

The only other tool Eissler acknowledged was the question, but its

role was essentially different from that of interpretation (p. 109).

He acknowledged that there were times when unavoidable departures from the ideal were necessary, but set up four conditions which must be met for the "parameters" to be valid introductions to the technique, though parameters were to be eschewed as much as possible as each increased the possibility of falsification of therapy by substituting obedience for structural change (p. 126). The four criteria for allowable parameters were:

- (1) A parameter must be introduced only when it is proved that the basic model technique does not suffice; (2) the parameter must never transgress the unavoidable minimum; (3) a parameter is to be used only when it finally leads to its self-elimination; that is to say, the final phase of the treatment must always proceed with a parameter of zero. (Eissler, 1953, p. 110)

The fourth condition, that the parameter must not give the transference a lasting direction, will be difficult to fulfill during the acute phases of the disease. If it has happened that a parameter has influenced the transference in a way which cannot be undone by interpretation, a change of analyst may become necessary. (p. 114 footnote)

A parameter was to be introduced only if, after its usefulness, it could be dispensed with and the treatment could proceed with the basic model (p. 110), and must never have been such that its effects on transference could not be abolished by interpretation (p. 113). The command by the analyst to face the feared object or situation for a phobic patient would constitute an allowed parameter. However, he decried any and all of the modifications made to treat schizophrenics.

It is impossible to demonstrate here the consequences which

must follow when the basic model technique is adapted to the necessities of such grave disorders as the schizophrenias always are...but I do want to stress that, despite the claim to the contrary by a few analysts, I am convinced that it has not yet been proved that schizophrenic patients ever reach a state in which they can be treated in accordance with the basic model technique. This is, to a certain extent, coincidental with a doubt that schizophrenic patients can be "cured" by psychoanalysis in that sense in which we commonly say neuroses can be cured. This statement is not to be construed as a denial of the effectiveness of psychoanalysis in the treatment of schizophrenic patients. (Eissler, 1953, p. 114)

He was particularly critical of those who used, then avocated the modifications because the clinician "has noticed subsequent disappearance of symptoms" (p. 113 footnote); or "it is a grave mistake to conclude that [a] measure has general validity because it has proved its usefulness under special conditions" (p. 105) While Eissler, beyond equivocation, had a point, he stretched it too far, and appears to have preferred theoretical parsimony over clinical efficacy in the treatment of schizophrenics.

The full weight of his wrath was reserved for Fromm-Reichmann though, as he criticized both her theoretical and technical modifications, particularly her switch to the "face-to-face situation." (p. 106)

Though not so rigid as Eissler, Stone in 1954 also came down squarely for an interpretationists position. He stated that any psychotherapeutic treatment which did not attempt to provide

to the maximum compatible with the situation the conditions necessary for a full-blown undistorted transference neurosis and therefore does not mobilize one, or which does not dissolve this neurosis or reduce it to the greatest extent

which the patient's structure and the therapist's skill permit, ultimately by genetic interpretations, should not be called analysis, even if the necessary formal aspects of the analytic situation are reproduced. For without these important processes, the profound reorganization of the personality which we associate with analysis, and in which the cure of illness is, in a sense, an incidental part of general economic change, can not occur. (Stone, 1954, p. 578)

Though Stone believed that one should not be rigid about details, he felt that any tangible deviation from neutrality should be handled within the general lines elucidated by Eissler (p. 574), (differing only with regard to criterion number 4, that any parameter must terminate prior to the end of analysis). Stone (p. 576) pointed out that this latter criterion automatically excluded the time limitation parameters which Freud had used with the Wolf Man. By 1958, certain of the interpretationists were differentiating among modifications, variations, or derivations of the basic interpretational model. (Greenson, 1958, p. 200; Loewenstein, 1958, p. 202) ²¹

Similarly, with respect to the second major controversy of the late 1940's and early 1950's, the dominant group within psychoanalysis, despite anomalies in child work and neurotic complementarities, refused to see family members. In 1954 Greenacre, with corroborations only slightly modified by Menninger (1958) strongly advised against seeing family

²¹ As will be seen in Chapter VII, this dispute was couched in terms of technique, the relationalists and the interpretationists obviously differed, with respect to paradigms, which served as the root of the disagreement, the methodological niceties being the logical extensions of each paradigm.

members and even physicians, as well as not giving or receiving information as this endangered trust and confidence in the analyst, jeopardized the analyst's integrity as s/he might be "prejudiced" by information other than the patient's free associations, and cast aspersions upon the motivation and training analysis of the analyst (cited by Grotjahn, 1960, pp. 25-26). Similarly Glover's 1955 The Techniques of Psychoanalysis (using information from questionnaires to analysts one year prior to World War II) reported that "all persons questioned see members of the family most unwillingly, and at the patient's request" (Glover, p. 322). Moreover, there was a

clear majority against analyzing members of the same family, at any rate at the same time. A minority would analyze them at different times, although some of these say definitely "not husband and wife". Three, however, replied unequivocally yes, and one of these stated that she had found it practicable to analyze a husband and wife. Another who tried this says it has some advantages, but the disadvantages are infinitely greater.

And in 1959, L.J. Saul, in an exercise in diplomacy stated that there existed advantages in meeting relatives, especially if they paid the bills or "in the rare cases which are obscure to the point of opacity." Saul was clearly not a rabid supporter of family inclusion.

With the official stance and dominant practice in psychoanalysis unsympathetic to seeing family members, it is little wonder that those analysts who did see family, did so with little self-proclamation. Galdstone, an analyst who did see family members, discussed the reasons he did so, and some of the reasons why it was a difficult modification for analysts to make. (in Ackerman, 1961, pp. 130-131). Gralnick

(1962, p. 516) stated that until recently, seeing, never mind treating, family members was regarded as "strange, if not heretical" and ascribed the reluctance to Freud's teachings and his difficulties with counter-transference in dealing with family members, as well as the legitimate difficulties in the early years, "in the treatment of the single individual." (p. 518) Framo and Boszormeny-Nagy (1965, p. XVI) summarize the difficulties, and consequences.

But psychologic investigation and treatment of the whole family had been a long-standing cultural and professional taboo;..Although the genius of Freud led to profound understanding of the intrapsychic world of the individual, his discouragement of involvement of family members in the treatment process of individuals and his view of the individual as a closed system established the practice of exclusion of the family in most forms of psychotherapy. It is understandable then, that, certain early family therapists were reluctant to report publicly their experiences to the professional community and that their experimenting with seeing families together was disclosed surreptitiously to their colleagues. (Framo and Boszormeny-Nagy, 1965, p. XVI)

Finally, Weakland (1972, pp. 132-133) pointed out that during the 1950's "there was very little family treatment anywhere and what there was seldom was mentioned publicly." In view of the disapproval of the main body of psychoanalytic practice, with its successes and prestige, there is little wonder the first steps within the framework were tentative. As might be expected, the revolutionary paradigm received its major impetus outside the framework.

Status of the situation in Kuhnian terms. By the late 1940's and early 1950's, psychoanalysis could be regarded as being in the midst of

of a Kuhnian crisis. The original paradigm set had been developed and revised by Freud until approximately 1914 and it provided the most clinically successful and theoretically powerful system available for the transference neuroses; there is little doubt that because of certain modifications, that remains the case. Then, precisely because of its successes and its position as virtually the only successful clinical framework for several decades, psychoanalysis began to be extended to phenomena other than those for which it had been designed. As predicted by the Kuhnian analysis, anomalies began to emerge (Stone talked of this extension of the psychoanalytic framework as having reached an "extreme development" by 1954).²²

This pattern emerged during the 1930's and 1940's in the traditional clinical populations that had the additional characteristic of complementarity with the neurotic features of another family member, particularly a spouse. Intentional or default extension of the techniques and formulations of psychoanalysis to the disorders of children, latent schizophrenia and schizophrenia resulted in anomalies during the 1940's and early 1950's, which struck at the paradigm as well as the theory and technique of the framework with regard to change process, primary technique of therapy and etiological formulation. Kuhn's schema allows some

²² The timing of the "extreme development" is interesting. This extremity occurred at about 1954 and the revolutionary DB family therapy paradigm was published just two years later in 1956. Is this sequence, so closely spaced in time, coincidental or will it be seen to be characteristic in Kuhnian analysis of other DMs and revolutionary paradigms?

of the difficulties of the psychoanalytic framework to be seen as structurally similar, and as resulting, not from the disconfirmation of the original theory as some had thought, but from its extension to areas it had not been designed for and which had been explicitly eschewed by Freud. The Kuhnian analysis also makes clear the method of extension--the technique becomes applied first to a new clinical area, then theory is imported to support findings, or to be necessarily modified in light of difficulties. The priority of technical rather than theoretical application has not received the attention that it requires, and in fact, has not been noticed at times. In 1954, discussing the extension of psychoanalysis to those other clinical phenomena, Anna Freud, mistakenly it appears, credits theory with priority.

...by no means all the variations of technique, which we find in the analytic field today, own their origin to special conditions in the cases under treatment. An equal, if not larger, number of them are occasioned, not by a change in the type of disorder treated, but by a change in the analyst's outlook and theoretical evaluation of familiar phenomena. The intimate interrelation between theory and practice in psychoanalysis is responsible for the fact that every development in theory results inevitably in a change of technique. To the measure in which classical psychoanalysis splits up into different schools of thought, the orthodox technique undergoes variations the value of which cannot be assessed except on the basis of the value of the theoretical innovations which have caused them. (A. Freud, p. 608; emphasis added)

In view of a Kuhnian analysis, it appears that one must disagree and regard the theoretical modifications as arising from the contingencies of attempted clinical application. As with Freud in the early 1890's, the applications of technique were directed towards the

alleviation of disorder--the cure was the goal--and the explanations regarding etiology and explanatory concepts of treatment were in the service of the cure.

By the 1940's sufficient time had elapsed between the attempted application of psychoanalytic technique and the emergence of patterns of difficulty for public reports to accumulate regarding difficulties, necessity for modification of technique, and finally for a new DM, based upon the Sullivanian interpersonal paradigm, to form. It appears that at approximately this point, awareness of crisis appeared. Awareness of the difficulties arising from extensions had existed among those concerned with such extensions, but the sense of crisis emerged only during the latter 1940's and early 1950's. The emergence of problems, presence of controversy and the examination of fundamentals (interpretationist position) characterized a period of crisis. Eventually, awareness of the crisis became explicit. In fact, in a paragraph encapsulating several of these elements in retrospect, Loewenstein stated,

In recent years, problems connected with variations of technique have aroused a great deal of interest among analysts. This may partly be due to the appearance of interesting modifications of psycho-analytic technique that have created considerable controversy and thus led, in turn, to renewed study of the base of classical technique...Moreover, this revival of interest is influenced by the 'widening scope' of the application of psychoanalysis, as well as by the increasing use of analytically oriented psychotherapy. Thus the need for a scrutiny of the rationale of all such techniques has become more acute. (Loewenstein, 1958, p. 202; emphasis added)

It is unclear, however, whether the strength of the interpersonal DM contributed to the sense of crisis or was responsible in some way for it. That is, did the sense of crisis arise because of the number and depth of the anomalies, or because of a combination of anomalies and powerful diverging framework addressing the anomalies? With an alternative or rival framework, do people of the established framework sense threat, or is the presence of anomalies sufficient? These questions, it seems cannot be answered in the present analysis. Briskman (1972) points out this sort of difficulty in the abstract.

Kuhn's theory, at its simplest and most elegant, portrays 'mature' sciences as evolving through a series of paradigms--normal science-crisis-and new paradigms. A fundamental weakness in this view, as pointed out by Feyerabend, is the failure to recognize that very often important arguments against the old paradigm may only come to light with the emergence of competing alternatives and that therefore the best way to provoke a 'crisis' may often be to offer an alternative in advance of any serious paradigmatic breakdown. Kuhn's chronology consistently runs from crisis to the emergence of alternatives; my point is that very often this process must be reversed. It is precisely for this reason that 'critical' (or, if one likes, 'crisis-oriented') epistemologies such as Popper's (1963) and Feyerabend's (1968) stress the important role of alternative theories in the growth of scientific knowledge. (pp. 89-90)

It appears that this is an important unresolved element in Kuhn's formulation.

In 1956, a paradigm was proposed that answered the anomalies and satisfied the Kuhnian criteria for a revolutionary paradigm. It responded to the general sense of crisis in psychoanalysis and specifically to the reciprocation of health and disorder found in schizophrenic

families and neurotic complementarities, and to the anomalies in the traditional treatment and formulation of schizophrenia, particularly the difficulties experienced in making sense of schizophrenic speech and the persistent lack of success in its treatment.

CHAPTER V

INCEPTION OF THE DOUBLE-BIND PARADIGM

To constitute a revolutionary paradigm in the Kuhnian sense, an ideal/technique would have to meet the following criteria: it would have to be a (1) problem solution which is (2) a new way of seeing things and which simultaneously serves (3) an analogue function; that is, it would serve as a Gestalt with which people could "see" new problems as subjects for the application of similar thinking and techniques as the paradigmatic idea. This problem-solution must be (4) sufficiently unprecedented to attract an enduring group of adherents away from competing models of scientific activity, yet also (5) sufficiently open-ended to leave problems for the new, re-defined group to address; that is, a paradigm would have to form a DM and leave it problems to address, and in fact, would (6) for a time provide model problems and acceptable solutions that pointed out a direction of development.

These are all necessary criteria that Kuhn has at one time or another emphasized. At other times, he has added supplementary points, some of which were proposed by Masterman originally, that have not appeared necessary, but that elaborate the paradigm concept and as such are useful. These supplementary characteristics specify that: the problem-solution (A) resolves those problems that constituted the anomalies (or at least some of them?), that it can be reached (B) deductively, during the course of (C) normal science activity directed

toward other interests and goals, by (D) "rank outsiders," that is, by individuals or a group outside the DM in which the anomalies have arisen; a consequence of the last three criteria is that the problem-solution is (E) cruder, and probably more concrete (than theorizing or techniques in the DM of anomalies).

It is unclear whether these supplementary characteristics apply to all paradigms to some extent, or, whether their presence means a particular paradigm is revolutionary; this might be especially relevant to supplementary characteristic "(A)", i.e., that the problem-solution specifically addresses the anomalies; are there problem-solutions that do not address anomalies, but merely address problems? Or would they, by definition, be normal science efforts; I would think they would be normal science in this case. In another case, however, what of those alternative theories that are said to induce crisis by their own existence without anomalies, of which both Feyerabend (1968) and Popper (1963) seem so enamored? These would not necessarily be addressing anomalies. This particular point--whether paradigms are necessarily revolutionary, is apparently still at issue.

To present this argument most clearly, the original DB formal statement will be explicated, then some of the subsequent modifications will be reviewed (in a manner similar to the review of Freud's formal paradigm statement). Then, the early work involved in formulating the DB paradigm will be examined, and an argument will then be made that the DB work constituted a revolutionary paradigm. The subsequent development of its DM, both sociologically as a research group, and

intellectually, will be reviewed in Chapter VI.

Initial Paradigmatic Statement:
"Toward a Theory of Schizophrenia"

The first formal paradigmatic statement took place in 1956 in an article titled "Toward a theory of schizophrenia" by Gregory Bateson, Don Jackson, Jay Haley, and John Weakland; the article was published in Behavioral Science rather than a family-therapy journal as there were no family journals in 1956.¹

The authors construed the article as "a report on a research project which has been formulating and testing a broad systematic view of the nature, etiology, and therapy of schizophrenia," (1956, p. 251) by focusing on the "basic family situation, and the overtly communicational characteristics of schizophrenia." (p. 262)

Base in communications theory. Their article began with an explanation of the theory on which the DB formulation was based: Their formulation was based on that

"part of communications theory which Russell has called the Theory of Logical Types" (Whitehead and Russell, 1910). The central thesis of this theory is that there is a discontinuity between a class and its members. The class cannot be a member of itself nor can one of the members be

¹In fact, the first family journal, Family Process, was a direct product of the DB group and issued its first copy for January 1962 with Jay Haley as editor. There had been "child" journals, but none to that time devoted to the clinical concerns of the family as a unit.

the class, since the term used for the class is of a different level of abstraction--a different Logical Type--from terms used for members. Although in formal logic there is an attempt to maintain this discontinuity between a class and its members, we argue that in the psychology of real communications, this discontinuity between a class and its members is continually and inevitably breached. (1956, pp. 251-252)

Examples of communication that involve multiple Logical Types include "play" for instance (among humans and at least the lower mammals). There are exchanges of signals that identify certain behavior as "play" and thus change meaning and consequences of the behavior. These signals (about the behavior)

are evidently of a higher Logical Type than the messages they classify. Among human beings, this framing and labeling of messages...reaches considerable complexity with the peculiarity that our vocabulary for such discrimination is still very poorly developed and we rely preponderantly upon nonverbal media of posture, gesture, facial expression, intonation, and the context of the communication of these highly abstract, but vitally important labels. (1956, p. 252)

Similarly, fantasy, humor, sacrament, and metaphor employ multiple Logical Types. Humor, for instance, was regarded as a method of exploring implicit themes in thought or relationship, with "the explosive moment" in humor occurring when the labeling undergoes "a dissolution and resynthesis," (p. 252), such that the punch-line compels a re-examination of the earlier labels that ascribed the statements to a certain mode (e.g., fantasy or literalness).

The falsification of such "mode-identifying" signals (e.g., "this is real", "this is only pretend") can also occur; they mentioned the artificial laugh, the confidence trick, the manipulative simulation of

friendliness. At times, these falsifications were perceived as occurring unconsciously and at times within the self, such that the person may conceal from him/her self, his own real hostility under the "metaphoric guise of play." (p. 252) (This point later becomes quite important with regard to schizophrenia)

Learning to learn, or learning sets (what Bateson's system refers to as Deutero Learning), involved multiple Logical Types and were also relevant to schizophrenia.

The shift to interest in schizophrenia. In 1954, Bateson's group became interested in schizophrenia because of its long acknowledged communicational oddities--its "word salad" quality and particularly the inappropriate metaphoric and literal qualities.

With regard to the communicational aspect of the paradigm, if their formal summary of symptomatology were correct, and if schizophrenia were essentially a result of family interactions,² then it should have been possible "to arrive a priori at a formal description of these sequences of events which would induce such a symptomatology." (1956, p. 253) Based on their perspectives in learning theory, particularly with respect to deutero-learning or learning sets, they

² Jackson's contribution to the paradigm was less communicational than interpersonal, and will be reviewed later during the analysis of paradigm development. For what appear largely historical reasons, during the explication of the DB paradigm and revisions, Jackson's component of the paradigm was more implicit than explicit. The reasons for this will be reviewed in Chapter VI.

deduced that these mental habits were the result of "characteristic sequential patterns" (p. 253) in the immediate environment, i.e., family. They eliminated "specific traumatic experience" (p. 253) during infantile development as an etiological agent, preferring to ascertain what contingencies would produce the communicational patterns they were seeing. Combining the emphasis on learning with the fact that the context of behavior is used as the "mode indicator," they deduced that characteristic patterns in the family "taught" the patient the mental habits (i.e., learning set) characteristic of schizophrenia.

That is, the patient "must live in a universe where the sequences of events are such that his unconventional communicational habits will be in some sense appropriate". (p. 253) They hypothesized that these characteristic sequences of events in the external experiences of the individual were responsible for what they perceived as an inner confusion in Logical Types. "For such unresolvable sequences of experiences, we use the term 'double bind.'" (p. 253)

Initial paradigmatic statement. The "necessary ingredients" of a double bind situation, as presented in their first paradigmatic statement, were:

1. "Two or more persons," one of which was designated the "victim."
 "We do not assume that the double bind is inflicted by the mother alone, but that it may be done either by mother alone or by some combination of mother, father, and/or siblings."
2. "Repeated experience," such that the double bind is a "recurring theme in the experience of the victim. Our hypothesis does not

invoke a single traumatic experience, but such repeated experiences that the double bind structure comes to be an habitual expectation." Or in Bateson's terms, the victim's deutero-learning, or learning set, was of double bind structure.

3. "A primary negative injunction," which may have either of two forms:

"(a) 'Do not do so and so, or I will punish you.' or (b) 'If you do not do so and so, I will punish you.'" The context of learning was based on avoidance of punishment rather than reward seeking and they indicated that there was perhaps no formal reason in terms of Logical Types, for this selection. (During the explication of the process developing the communicational part of the paradigm, the possible origin of this choice will be shown to lie in the personal correspondence between Gregory Bateson and Norbert Weiner.) The group assumed that punishment could be either the withdrawal of love or the expression of hate or anger, or worst, "the kind of abandonment that results from the parent's expression of extreme helplessness."

4. "A secondary injunction conflicting with the first at a more abstract level, and like the first enforced by punishments or signals which threaten survival." This secondary injunction is more difficult to explain than the first for what they state to be two reasons.

The first is that it is commonly conveyed to the child by non-verbal means. "Posture, gesture, tone of voice, meaningful action and the implications concealed in verbal communication may all be

used to convey this more abstract message." Secondly, this secondary injunction may impinge on any element of the primary message and may therefore take a wide variety of forms and directions. For example, "'Do not see this as punishment'; 'Do not see me as the punishing agent'; 'Do not submit to my prohibitions'; 'Do not think of what you must not do'; 'Do not question my love of which the primary prohibition is (or is not) an example'; and so on." The discernment of these secondary injunctions is made more difficult at times when the double bind is inflicted by not one person, but two, wherein, for example, one parent may negate at a more abstract level, the injunctions of the other.

A further difficulty not mentioned by the four authors might be mentioned. Since the secondary injunction is at a more abstract level, and serves a meta-communicational function, it is by definition and function more abstract and "once removed" from the ostensible content of messages. As such, its presence and role would of course be more difficult to discern.

5. "A tertiary negative injunction prohibiting the victim from escaping the field. In a formal sense it is perhaps unnecessary to list this injunction as a separate item since the reinforcement at the other two levels involves a threat to survival, and if the double binds are imposed during infancy, escape is naturally impossible." They further allude that, in some cases, escape from the fields is precluded by processes which are not "purely negative, e.g., capricious

promises of love, and the like."

6. "Finally, the complete set of ingredients is no longer necessary when the victim has learned to perceive his universe in double bind patterns. Almost any part of a double bind may then be sufficient to precipitate panic or rage. The pattern of conflicting injunctions may even be taken over by hallucinatory voices... [quoted and paraphrased from Bateson, Jackson, Haley, and Weakland, 1956, pp. 253-254; emphases theirs]

Probably the best known illustration (and analysis) of a double bind comes from this 1956 paradigmatic paper. Bateson, et al. provide a clinical illustration which at least one of them apparently saw (Though their analysis is rather long, it will be presented in toto, as the double bind formulation is not particularly easy to recognize in behavior unless one has had a good deal of experience at it, as an aid to understanding the power of the situation and its implications for the patient and to explicate clearly at least one double bind analysis to make clear the formulation what all subsequent fuss was about; without a clear understanding of its unresolvable quality in combination with the impossibility of "doing nothing," the etiological significance of the double bind will remain pallid.

An analysis of an incident occurring between a schizophrenic patient and his mother illustrates the "double bind" situation. A young man who had fairly well recovered from an acute schizophrenic episode was visited in the hospital by his mother. He was glad to see her and impulsively put his arm around her shoulders, whereupon she stiffened. He withdrew his arm and she asked, "Don't you love me any more?" Then then blushed, and she

said, "Dear, you must not be so easily embarrassed and afraid of your feelings." The patient was able to stay with her only a few minutes more and following her departure he assaulted an aide and was put in the tubs. (1956, p. 259)

Clearly, the intensity of his upset could have been averted if he had been able to meta-communicate: "Mother, it is obvious that you became uncomfortable when I put my arm around you, and that you have difficulty accepting a gesture of affection from me". This, however, is exactly the point--the person cannot meta-communicate due to 1) training and the resulting learning set and 2) intense dependency which is fostered by the double bind patterns. He could not meta-communicate but was forced to deal with the communication of the mother, and listen to her comments about his communication and behavior. The logical vicissitudes of the sequence are bewildering to the observer (and will be reviewed); to one of the participants, the sequence was obviously pathogenic.

The logical complications for the patient in that perhaps thirty second interchange include:

1. The mother's reaction of not accepting her son's affectionate gesture is masterfully covered up by her condemnation of him for withdrawing, and the patient denies his perception of the situation by accepting her condemnation.
2. The statement, 'Don't you love me any more?' in this context seems to imply:
 - (a) 'I am lovable.'
 - (b) 'You should love me and if you don't you are bad or at fault.'
 - (c) 'Whereas you did love me previously you don't any longer', and thus focus is shifted from his expressing affection to his inability to be affectionate. Since the patient has also hated her, she is on

- good grounds here, and he responds appropriately with guilt, which she then attacks.
- (d) 'What you just expressed was not affection,' and in order to accept this statement the patient must deny what she and the culture have taught him about how one expresses affection. He must also question the times with her, and with others, when he thought he was experiencing affection and when they seemed to treat the situation as if he had. He experiences here loss-of-support phenomena and is put in doubt about the reliability of past experience.
3. The statement, 'You must not be so easily embarrassed and afraid of your feelings,' seems to imply:
- (a) 'You are not like me and are different from other nice or normal people because we express our feelings.'
 - (b) 'The feelings you express are all right, it's only that you can't accept them.' However, if the stiffening on her part has indicated 'these are unacceptable feelings,' then the boy is told that he should not be embarrassed by unacceptable feelings. Since he has had a long training in what is and is not acceptable to both her and society, he again comes into a conflict with the past. If he is unafraid of his own feelings [which mother implies is good], he should be unafraid of his affection and would then notice it was she who was afraid, but he must not notice that because her whole approach is aimed at covering up this short-coming in herself.

The impossible dilemma thus becomes: 'If I am to keep my tie to mother I must not show her that I love her, but if I do not show her that I love her, then I will lose her.'
(1956, p. 259)

Their example is particularly vivid for me, as, soon after first reading the 1956 report, I saw a very similar interchange, with similar outcome. A hospitalized man in his early 30's, with a diagnosis of catatonic schizophrenia, was visited by his father, a man whose occupation had self-conscious "he-man" characteristics. Father came on

the unit, hugged his son and kissed him; the son froze and did not respond. Father said, "What's the matter, you don't love your old man?" Son made placating gestures, but couldn't speak as he'd been effectively mute for three years. Father stayed approximately ten minutes, standing in the hall talking, then began saying his goodbyes. Son hugged and kissed him and father pushed his son away, saying, "What are you, 'queer' or something?" Son stood there, not answering and father questioned, "Well?", forgetting that his son had been mute since father had burned his son's journals and a manuscript returned by an editor, saying that it was undignified for his son to be "emoting all over the place."

Thus, the son could not do right, could not do nothing, and could not meta-communicate (on one level because that was emoting and therefore devalued and on another level, because he was mute and "could not." Of course, when he had not been mute, when he'd written those journals and manuscript, he had been rendered mute by their destruction). Upon his father's departure, he became assaultive, and self-destructive, and was placed in temporary isolation, emerging the next day as if nothing had happened.

The induction of double bind. The mothers of schizophrenics³ were

³ Despite disclaimers to the effect that they held no one in particular responsible for inception of double binds and hence, schizophrenia, the 1956 article repeatedly lays the blame at mother's doorstep, in formulations and illustrations; they correct the imbalance somewhat in subsequent revisions, though because of the 1956 article's enormous audience, the mother-as-"binder," child-as-"victim", father-

hypothesized to be simultaneously expressing at least two orders of messages, roughly characterized as a) withdrawing or hostile behavior which is aroused whenever the child approaches her, yet b) simulated loving or approaching behavior which is aroused whenever the child responds to her hostile or withdrawing behavior, as a way of denying what she is withdrawing. The dynamic problem is the mother's need to control anxiety in the face of intimacy, which she does by controlling the distance (or closeness) between her and the child.

It's also obvious that any child exposed to this sort of mothering with any regularity and with little consistent mothering functions from elsewhere (relative, housekeeper, neighbor), would develop particularly persistent, but schizoid, ways of attempting to gain that affection, regard, comfort--any of the guises under which children receive the steady love that is their necessity, and right. In the face of these efforts, the mothers would, of course, escalate the double binding, par-

as-"innocent bystander" Gestalt has unfortunately remained powerful. The 1956 preoccupation with mother as perpetrator is being elucidated in the interests of historical accuracy rather than clinical veracity, and as a baseline against which to gauge subsequent paradigmatic revisions. The shift from dyadic preoccupation in double bind establishment, to three part, then family involvement, was quite important in the DM development. This should not be construed to mean, however, that the original paradigmatic statement was dyadic; it was clearly dealing in systems terms with family dynamics; for ease of understanding, their own as well as readers', the project's first statement dealt with the simplest version of the interactive system, the two-person system. As Jackson's tenure in the group increased, the influence of homeostatic processes in the family became more prominent in the formulation.

ticularly with respect to approach-avoidance.

The most important point the project members wanted to bring out was that in this oscillating process, the mother's "loving behavior is then a comment on (since it is compensatory for) her hostile behavior and consequently it is of a different order of message than the hostile behavior--it is a message about a sequence of messages. Yet by its nature, it denies the existence of those messages which it is about, i. e., the hostile withdrawal." (1971, p. 12) It was necessary to hypothesize, and they did, that hostility and aggression could not be admitted to by these mothers. (Obviously psychoanalytic-thought had sufficiently pervaded the general culture to be used, in part, in almost any psychiatric formulation, particularly with respect to psychological processes, and defenses, e.g., denial, though not so much for structure and content of its formal theory.)

Because of her own difficulties, the mother uses the child's response to affirm that her behavior is loving, and since it, in fact, is only simulated lovingness, "the child is placed in a position where he must not accurately interpret her communication if he is to maintain his relationship with her." (1956, p. 257) In fact, the child must not discriminate among the orders of messages, here the difference between simulated feeling (one Logical Type) and real feelings (another Logical Type). In effect, the child comes to distort his perception of the meta-communicative signals. For instance,

if mother begins to feel hostile (or affectionate) toward her child and also feels compelled to withdraw from him, she might say, "Go to bed, you're very tired and I want you to get your sleep." This overtly loving statement is intended to deny a feeling which could be verbalized as "Get out of my sight because I'm sick of you." If the child correctly discriminates her meta-communicative signals, he would have to face the fact that she both doesn't want him and is deceiving him by her loving behavior. He would be "punished" for learning to discriminate orders of messages accurately. He therefore would tend to accept the idea that he is tired rather than recognize his mother's deception. This means that he must deceive himself about his own internal state in order to support mother in her deception. To survive with her he must falsely discriminate his own internal messages as well as falsely discriminate the messages of others. (1956, p. 257; emphasis added)

Such a process would help one to understand the sense of bewilderment or vague puzzlement often encountered among schizophrenic patients; similarly, the tendency to become alienated from one's self to maintain a vital relationship is given some etiological formulation. The just quoted DB passage is reminiscent of the schizophrenic patient quoted by Powdermaker (1952), who felt at times that a quarrel meant either the giving up of self, or loss of the relationship.

It is the inability to hold their own with authority figures who seem illogical or otherwise ununderstandable that has made life so difficult for the patients in the first place...The need of the patient for someone who will try to understand what he is endeavoring to communicate is made clear by the remark of a patient: 'If the others don't agree it means you're wrong - it takes so much strength to be a minority of one.' (1952, p. 67)

A DB formulation would emphasize the bind between integrity of his own perceptions and possible loss of relationship, as well as the obvious inability to metacommunicate regarding whether disagreement

necessarily implied that the patient were wrong.

The tendency toward internal deception to maintain the relationship is abetted by the picture of benevolence; the mother is expressing overt concern over the "fact" that the child is tired. The easiest path for the child is to accept mother's simulated loving behavior, and any inclinations toward deciphering or interpreting what is going on, are undermined. "Yet the result is that the mother is withdrawing from him and defining this withdrawal as the way a loving relationship should be." (p. 257)

However, accepting mother's simulated loving behavior as real is also no solution for this child as, if he should approach her (having made the false discrimination) she would of course, be induced to withdraw; if he then withdrew, she would interpret this as a reflection on her status of a loving mother, and would either approach, or withdraw to punish the child. If he then re-approached, she would be compelled to distance. "The child is punished for discriminating accurately what she is expressing and he is punished for discriminating inaccurately-- he is caught in a double bind." (quoted and paraphrased from 1956, p. 257)

To say that the child "should" simply desist in his efforts is to miss the point. First, relationally, the child must make these efforts-- because he cannot give up what he has never had and continues to require, and because he is responding to the needs of the mother and, her active signals are constructed such that he cannot not act.

Attempts to deal with double-bind situations. A variety of means can be used by the child to attempt to escape from the situation. One is to rely for the necessary relational ties on someone else--the aforementioned relatives, housekeepers, etc., or the father. A peculiarity of schizophrenic families is the insubstantiality of the fathers. The "dominating mother" has become a stereotype by now with regard to schizophrenic families. To turn it around, according to the DB paradigm. it is because families have one parent (here the mother) who double binds and another parent who is insubstantial, or weak, or passive, or emotionally absent (here the father), that one of the children is at risk for schizophrenia. If the child could gain relational security with that other parent, the pernicious effects of the binding parent would be greatly mitigated, particularly with respect to their emotional consequences.

The fathers should not be regarded as totally oblivious to the child's problems in relation to mother. Father is also in a difficult position. If father recognizes the difficulty and supports some of the child's efforts, particularly with respect to the mother's deceptions he would then have to recognize the nature of his own relationship with her, which he could not do and maintain that relationship (p. 258) To "help" the child would tilt the homeostatic balance in the triad and tilt father out of his, at least, tolerable position. The homeostatic processes in the family preclude intervention or real availability by the father.

As to the benign presence of relatives, neighbors, etc., another peculiarity of schizophrenic families is their defensive isolation in the world. Any attempt by the child to get its relational needs met outside the family would be interpreted by the binding parent as a reflection on their "lovingness" and would produce anxiety, thereby setting into play the various homeostatic processes to "right" the situation and withdraw the child from the outside contact. While this is particularly true for families with paranoid trends, it is characteristic of nearly all schizophrenic families. (See, for instance, F.D. Laing's Sanity, Madness and the Family, 1964, for several vivid pictures of these families' isolation). Because of this isolation, it is highly improbable that the child can gain that consistent relational contact outside the family.

Meta-communication by the child (another avenue of escape) is extremely difficult; for children until they begin school, the family constitutes their reality.⁴ This is not the only way the family functions for the child. Additionally, the family identifies objects and processes, and negates others as non-existent or trivial; it establishes the nature of reality, humans, and time in implicit as well as explicit ways and

⁴The forms of expression here, and some of the ideas, were drawn from Mehan and Wood (1975) who discuss "reality constituents." This section, regarding the child's reality, seems to owe a great deal to Mehan and Wood, though it is difficult to specify specifically where.

in fact, the implicit knowledge and attitudes are the most difficult to recognize and modify. Upon going to school, most children first encounter reality systems different from their own in a way that must be reckoned with. At school, new systems are encountered and for some children, according to Sullivan, constitute a saving grace by giving them relief and different realities from a malignant family reality system. But it is asking too much of the pre-school child to stand outside the family reality and comment, of all things, on its parent; the form of perceptiveness required at times boggles trained adult minds (hence the wide use of video-taping to study double binds). It is too much to expect that a person in the child's position could so comment.

But, what if by fluke, or genius, or unmitigated bad luck, the child should somehow meta-communicate? "Bad" luck here, because in this set of processes, the mother would regard his comment as an "accusation that she is unloving and both punish him and insist that his perception of the situation is distorted." In effect, the punishment is double-punishment by withdrawal and hostility, and insistence upon alienation of his perceptions. In logical terms, the child received only one punishment for accuracy or inaccuracy; for meta-communication, he received two, a situation which one assumes would reduce the frequency of his meta-communications.

Also, by preventing the child from using the meta-communicational mode, the mother cuts off the child's access to correction of perceptions of communicative behavior. The ability to communicate about

communication, and to test perceptions about communication is essential for successful social intercourse (p. 258) It is this meta-communicational level it was felt that schizophrenics were perceived as unable to use (for instance, they confused the literal and the metaphoric in their own speech,⁵ and the double bind relational "training" was proposed as its source.

Psychosis was viewed then by project members as a final attempt to deal with double binds. However, by "adopting" or resorting to psychosis, the individual has opted to deal with double binds within the double bind framework, rather than by stepping outside of it (as in meta-communication for example). But, psychosis is a way of avoiding the worst of the emotional consequences of being bound.

It is not only safer for the victim of a double bind to shift to a metaphorical order of message, but in an impossible situation it is better to shift and become somebody else, or shift and insist that he is somewhere else. Then the double bind cannot work on the victim, because it isn't he and besides he is in a different place. In other words, the statements which show that a patient is disoriented can be interpreted as ways of defending himself; either he does not know that his responses are metaphorical or cannot say so. (1956, p. 255)

⁵ A patient may wish to criticize his therapist for being late for an appointment, but he may be unsure what sort of a message that act of being late was. The patient cannot say, "Why were you late? Is it because you don't want to see me today?" This would be an accusation and so he shifts to a metaphorical statement. He may then say, "I knew a fellow once who missed a boat, his name was Sam and the boat almost sunk...etc.," Thus he develops a metaphorical story and the therapist may or may not discover in it a comment on his being late. (Bateson et al., 1956, p. 256)

The more frequently encountered and easily differentiated forms of such a psychosis (paranoid, hebephrenic and catatonic) were re-interpreted in DB terms. For instance, paranoid schizophrenia is cast into communicational terms.

Given this inability [because of problems with meta-communication] to judge accurately what a person really means and an excessive concern with what is really meant, an individual might defend himself by choosing one or more of several alternatives. He might, for example, assume that behind every statement there is a concealed meaning which is detrimental to his welfare. He would then be excessively concerned with hidden meanings and determined to demonstrate that he could not be deceived--as he had been all his life. If he chooses this alternative, he will be continually searching for meanings behind what people say and behind chance occurrences in the environment, and he will be characteristically suspicious and defiant. (pp. 255-256)

Similarly, hebephrenia or catatonia were re-case; the particular styles adopted in psychosis were portrayed as "like any self-correcting system which has lost its governor; it spirals into never-ending but always systematic, distortions." (p. 256)⁶

Therapeutic DBs. This difficulty in communication, in point of fact, is one of the reasons verbal therapies proved unsuccessful with schizophrenia, and why "therapeutic double binds" were initiated. Therapeutic double binds essentially fought fire with fire. The acknowledged

⁶ This process of re-interpreting the phenomenon of interest is characteristic of revolutionary paradigms; it is a method of re-casting the phenomenon into terms its Gestalt can accommodate.

difficulties of doing traditional verbal therapy with schizophrenic patients, the frequency of relapses, and the sense that the more things changed in these families, the more they stayed the same impelled the DB project members to fight fire with fire; if the family processes were so powerful and binding that rational approaches did not suffice, they would try an irrational approach, i.e., to double-bind the double-binding families, in the interest of the patient.

Though the psychoanalytic paradigm had been developed to cure hysteria, with etiological explanations secondary and in the service of change techniques, the DB paradigm was formulated to solve the problem of the etiology of schizophrenia as the issue had been raised by anomalies emerging in psychoanalytic treatment. Treatment using the DB concept was a secondary concern, although, gradually the emphasis changed and DB material is currently used more for treatment than investigation. Ironically, at points Freud had pointed out that psychoanalysis was a better investigative than therapeutic process. An interesting question might be whether, in clinical DMs, it is usual for the primary emphasis to shift in time; that is, for a paradigm developed to cure (a clinical application), later to emphasize investigation, and for these paradigms established primarily for investigation, to later emphasize clinical application. This might be a common developmental pattern for clinical DMs, which necessarily have the active dual foci of theory and application. Can the operant conditioning paradigms as elaborated by various clinicians, for example, be said to change emphasis in time?

The therapeutic use of the DB pattern was regarded in 1956 as having only "therapeutic implications" and was the last section covered in the article. Interest centered, not so much in doing therapy, as explaining it in DB terms, as "a context of multilevel communications." (1956, p. 263) Double binds, with all their formal characteristics, were perceived as created by and within the psychotherapeutic setting and in hospital settings. For instance, picking up on the common concerns of hospital settings and mothers of schizophrenics with the issue of benevolence, they assumed that whenever a hospital (or unit) system was organized for hospital purposes, and the patient was told that certain actions were for his benefit, then a double bind had been perpetrated.

Also, the project members felt that the DB and the emphasis on communicational analyses had potential for innovating therapeutic techniques, and in particular in making the invocation of benign or therapeutic binds by the therapist, a matter of intention and skill, rather than intuition and luck. With elucidation of the DB "ingredients" it would be possible for DB's to be formulated systematically, and when needed, by therapists.

The difference between a therapeutic DM, and a malignant one was based on the fact that the therapist was not involved in a "life and death struggle himself..." and could therefore set up binds to move patients in positive directions, then assist them in "emancipation from them." (p. 264)

Homeostatic aspect. In the 1956 article, the homeostatic aspect in which implicit family processes maintained the status quo, was much less elaborated than the communicational. It was not until just prior to Weakland and Jackson's 1960 paper on three-person interaction that the homeostatic aspect was explicitly elaborated. But the homeostatic material had been necessary to account for the dependency of the patient, the passivity of the non-binding parent, the complementarity in disorder, for the relapses into psychosis and old, unpleasant double bind patterns. The concept of homeostasis provided the relational context, as it was only within relationships that double binds could carry their powerful emotional impact.

Homeostatic processes, unless dealt with, could sabotage, or prematurely terminate therapy, as was often the case with schizophrenia. In discussing the case of a young woman schizophrenic, the DB group reported that two times during therapy, the patient's mother showed intense emotion: once when relating her own (previously concealed) hospitalization for a psychotic break and during her last visit

when she accused the therapist of trying to drive her crazy by forcing her to choose between her daughter and her husband. Against medical advice, she took her daughter out of therapy. (p. 260)

Homeostatic elements would imply that daughter was improving (as daughter was improving, mother was worsening, i.e., mother was being "driven crazy"), that the therapist had made moves towards getting husband and wife closer ("forcing" mother to choose) and that therapy

had disrupted the homeostatic balance severely enough that mother would react by removing daughter from therapy against advice, and restore the usual balance. They further pointed out:

The father was involved in the homeostatic aspects of the intra-family situation as the mother. For example, he stated that he had to quit his position as an important attorney in order to bring his daughter to an area where competent psychiatric help was available. Subsequently, acting on cues from the patient (e.g., she frequently referred to a character named "Nervous Ned"), the therapist was able to elicit from him that he had hated his job and for years had been trying to "get out from under." However, the daughter was made to feel that the move was initiated for her. (1956, p. 261)

And again, the theme of ostensible benevolence emerges. Adding other family members multiplies the variety of maneuvers the system can employ to maintain its homeostatis (this, ironically, makes the work of the family therapist easier as [among other reasons], the situation is more fluid than the relatively rigid homeostatis between two or even three people).

Another facet revealed by the relational approach indicated that particularly conflictual situations for the patient were related to areas important to mother's self-esteem (p. 260) These areas apparently were most subject to double binding and thus the emotionality connected to them by the patient was greater than for most issues. According to their formulation, the communicational system functions to protect "mother's security..." and by inference, family homeostasis. This being the case, when therapy helps the patient be less vulnerable to

this form of communication (and control), anxiety rises in the mother, and attempts are made to reinstate the usual homeostasis. The psychosis was an adaptation to the communicational system, and allowed the patient certain degrees of freedom, especially in the use of metaphor and disclaimer, almost in the manner of court jester. Psychosis is also a way of side-stepping some of the emotions which are engendered by the double binding including helplessness, exasperation and rage.

Early Revisions and Modifications

Within a short time after its 1956 publication the DB paradigm received a great deal of attention. Watzlawick (1963) cites almost ninety articles addressing, or taking into account, the DB ideas between 1957-1961. This number is indicative of widespread interest, particularly as the article was not published in a psychotherapy journal, and was therefore not immediately apparent to those people with an interest in family work (of which there were only a small number in 1956). Since then, the DB paradigmatic statement and its subsequent elaborations have become one of the dominant family therapy frameworks.

During the years immediately following the paradigmatic statement, several points of revision were introduced. First the authors attempted to clarify what they regarded as misunderstandings of the concept, and to bring into their framework whatever alternate explanations were being ascribed to the DB ideas. Thus, certain points became elaborated in the interests of clarity/maintenance of control

over their definitions. Secondly, there was an explicit shift from "binder-bindee" to a concept of "mutual binding." This reflected a shift to increasing emphasis on the relational and homeostatic aspect of the paradigm (rather than the communicational). This implied the process of three-party interaction; when Weakland (1960) developed the three-party idea more fully, it in turn strengthened the homeostatic aspect of the paradigm.

Attempts to maintain paradigmatic purity. Early revisions attempted to elucidate obscure points and defend the integrity of the DB paradigm. The DB's historical priority was challenged (Devereux, 1959) as a re-discovery of Devereaux's Sociological Theory of Schizophrenia (Watzlawick, 1963), and its central concepts were at times, mis-identified as "equivalent to any kind of contradictory communication, or an approach-avoidance conflict, or difficulty in discriminating messages, or any number of other things" (Abeles, 1976, p. 115).

The concept of "paradox" and "levels of communication" were the ones most subject to misinterpretation. For example, Watzlawick (1963) cites several authors who regarded the DB as equivalent to ambivalence. As Watzlawick pointed out, ambivalence was usually regarded as the simultaneous presence of mutually contradictory emotions, in particular, love and hate; the concept was intrapsychic rather than interactional. By remembering that the DB addressed multiple levels of communication, it became clear that one emotion could qualify the other, from some other level, usually by using the more abstract level as qualifier.

Similarly, paradox was often mistaken for simple contradiction, an error the DB adherents were at pains to correct.

It is essential to distinguish between paradox and other kinds of contradictions and incongruencies since the double bind is so often interpreted as meaning inconsistent communication or contradictory messages and the like. Unless such definitions further specify that the contradiction occurs between different levels of abstraction, or different logical types, the definition is one of simple contradiction rather than paradox. A qualitatively different feature of paradox is its reflexiveness—the invalidation of its referents by itself—so long as one remains conceptually within the frame posed by paradox.

Watzlawick, who has repeatedly reminded double bind investigators of the importance of this distinction (Watzlawick, 1963, 1965; Watzlawick, et al., 1967) offers the following example to illustrate this crucial distinction: With a pair of contradictory orders such as "Stop" and "No Stopping Anytime," one may choose to obey one or the other, though the unchosen will of course be disobeyed. With paradox, however, there is essentially no choice, though there is the illusion of choice: e.g., a sign which reads "Ignore this Sign." In this illusion lies the difficulty, since it is not simply that you will be wrong whatever you do, but that you cannot really do anything at all. (Abeles, 1976, p. 118)

The reflexive feature of a paradox, "the invalidation of its references by itself" provided the logically inescapable quality for the individual. For, if performing either injunction necessarily disobeys the other, the individual cannot do something, yet neither can he not do something. Thus, in a family, the demand to "Be spontaneous" is a paradox, as the person cannot not disobey. If he "obeys" and attempts to be spontaneous, that is manifestly not spontaneity but rule-following. There is essentially no way in which can respond to this injunction, as it would involve a paradox, with the fulfillment of one

referent necessarily invalidating the other. If one follows orders and "is spontaneous", he is not; if one does not follow orders to be spontaneous, again he is not. With the addition of the relational component, such a paradox gains its emotional intensity for the "victim". Abeles (1965, p. 116), for instance, has recognized this aspect of DB paradox and emphasized "the experiential effects of paradox within an intensely important relationship." This emotional quality is not present in the same manner with simple contradiction-only with paradox.

Haley (1959) addressed the paradoxical rather than contradictory quality of behavior by contending that "one cannot not qualify a message. A person must speak a verbal message in a particular tone of voice, and if he says nothing, that, too, is qualified by the posture he presents and the context in which his muteness appears." (p. 323) The qualification of messages is literally inescapable and is dependent upon the multiplicity in levels of communication.

Bateson addressed yet another misconception of paradox in 1926 - the misinterpretation of paradox as failure in discrimination,

In the well-known experiments in which an animal subject is reduced to psychotic behavior by first training the subject to discriminate, e.g., between an ellipse and a circle and then making the discrimination impossible, the "trauma" is not as is commonly stated, the "breakdown of discrimination" but is the breakdown of that pattern of complex contingencies which the experimenter had previously taught to the animal. As I see it, what happens at the climax of the experiment is that the animal is penalized for following a deeply unconscious and abstract pattern which the psychologist previously rewarded. It is not that the animal cannot discriminate, it is that the animal is put in error when he thinks that this is a context for discrimination.

Increasing emphasis on relational and homeostatic elements.

Shift to concept of mutual binding. The 1956 statement made explicit use of "binder" and "victim." By 1958, Weakland and Jackson were documenting the shift to a position where, though there appeared to be a victim, both or all members of a family were perpetrators and also victims of DBs.

Bateson, Jackson, Haley, and Weakland (1) postulated that part of the etiology of schizophrenia is a communication sequence they labeled "a double bind." A double-bind relationship, most simply, can be described as a hostile dependent involvement where one of the parties insists on a response to multiple orders of messages which are mutually contradictory, and the other (the schizophrenic patient-to-be) cannot comment on these contradictions or escape from the situation. Obviously, a double-bind relationship can exist only within a special family or group relationship, since, for example, a child could break out of a double-bind situation with his mother if his father was capable of handling such contradictory multi-leveled messages and thus setting an example and offering support to the child. (1958, pp. 88-89)

This revised short-form definition emphasized both the relational elements, and the necessity of the homeostatic element ("if his father were capable..."). It also implied a later revision--Weakland's three-party formulation.

Weakland and Jackson perceived the "binder" as being as much a victim because s/he is as caught up in the maneuvers and consequences as the child. With this revision, the DB was precluded from being a one-way relationship and was shifted to an interactional sequence wherein all parties shared important similarities of behavior (1958, p. 111).

Mutual binding was a logically inevitable revision; as Abeles (1976, p. 119) points out, attempting to respond to a paradox within the terms set up by the paradox itself "invites, in Russell's language, vicious circle reasoning." According to the authors, any such response would necessarily be as paradoxical as the situation which elicited it.

To illustrate, consider the entire class of injunctions commanding behavior which by definition can only be spontaneous, e.g., "Be independent." The basic injunction is the X be independent. The statement is an order, and thus evokes a response which will in that context be a response to an order. It is paradoxical in that independence cannot be ordered; to obey is to disobey. The injunction implies alternatives which are nonexistent, it implies by its assertion that it is somehow possible to respond with the requested behavior. Any response within that context is invalidated by being subject to redefinition at another level. (Abeles, 1976, p. 119)

Thus, any attempt to respond (including, in this framework, the attempt to not respond) within the confines of a paradox, becomes necessarily paradoxical in turn and binds the previous "binder." (This revision, or shift in emphasis, led to the concept of spirals of binding, which will be reviewed shortly).

Haley, a year later (1959a) addressed the mutual binding concept in control, rather than logical or purely relational, terms. Working on the assumption that, if a child learned to relate to people in a relationship with parents who constantly induced him/her to respond to paradoxical messages, the child might well learn to relate in these terms, with parents and others. Haley then inferred that "the control of the definition of relationships would be a central problem in the

origin of schizophrenia." (1959a,p. 323)⁷

Haley provided a behavioral illustration of both mutual binding and the control issue (1959a,pp. 330-331).

Preliminary investigations of schizophrenic patients interacting with their families suggest that the patient's way of qualifying his statements incongruently is a habitual response to incongruent messages from his parents. As an illustration, suppose that a mother said to her child, "Come and sit on my lap." Suppose also that she made this request in a tone of voice which indicated she wished the child would keep away from her. The child would be faced with the message, "Come near me," qualified incongruently by the message, "Get away from me." The child could not satisfy these incongruent demands by any congruent response. If he came near her, she would become uncomfortable because she had indicated by her tone of voice that he should keep away. If he kept away, she would become uncomfortable because she had indicated she would become uncomfortable because after all she was inviting him to her. The only way the child could meet these incongruent demands would be to respond in an incongruent way; he would have to come near her and qualify that behavior with a statement that he was not coming near her. He might, for example, come toward her and sit on her lap while saying, "Oh, what a pretty button on your dress." In this way he would sit on her lap, but he would qualify this behavior with a statement that he was only coming to look at the button. Because human beings can communicate two levels of message, the child can come to his mother while simultaneously denying that he is coming to her--after all, it was the button he came to be near.

By saying, "Come sit on my lap," in a tone of voice which

⁷ This is Haley's earliest major discussion of the issue of control in relationships, a topic of continuing interest for him throughout the life of the DB project. It is, after all, a possible interpretation of the DB paradigm, which implies that the world is composed of opposite dichotomies in hierarchy. If at times, one can neither do, nor not do, a thing, the issue of control is implied.

indicates, "Keep away from me," the mother is avoiding defining her relationship with the child. More than that, she is making it impossible for the child to define his relationship with her. He cannot define the relationship as one of closeness nor can he define it as one of distance if he is to satisfy her incongruent demands. He can only manifest incongruent messages himself and thereby avoid defining his relationship with her. [emphasis added]

Weakland's 1960 paper on three-party interaction dealt even more directly with mutual binding. Weakland presented a summary of their original statement, indicating that it emphasized a pattern of messages sent by a "binder." In our early work there was an urgent need to emphasize this point, to insist, against a climate of opinion focused either on fantasy or physiology, that real people were giving real, observable messages that were provocative of schizophrenic responses." (p. 375)

Weakland proceeded to modify the earlier statement to take into greater account the duality and paradoxical qualities of the situation, as responded to and perpetuated by all its members. He remarked that the total sequence then must be regarded to have the form of a larger and more encompassing double bind, which progressively aggravates the situation. (p. 376)⁸

⁸This is not to imply to the topic of mutual binding withered away. Abeles states that it was later addressed, after dissolution of the formal project, by Watzlawick's review in 1963, by Bateson, et al. in their 1963 summary review, and by Jackson in 1965 (1965, p. 116).

Double bind spirals. The issue of double bind engendered double bind in return, contributing to the overall family communicational pattern, was a natural corollary of the shift to mutual binding. A number of people recognized the spiraling quality of these sequences.

Weakland and Jackson addressed it initially in 1958, referring to "reverberating cyclic sequences" and "mutual uneasiness" (p. 115), as well as emphasizing that in the context of the DB, "a troubled relationship begets further troubled relationships." (1958, p. 114)

Weakland extended the treatment in his 1960 "three-party" paper.

Whenever any such message of concealment, denial, or inhibition is added to reinforce an original double-bind communication, the combination produces another double-bind structure, on a wider scale. For example, when the occurrence of a pair of incongruent messages is followed by a further message denying that there was any contradiction, this combination comprises another pair of incongruent messages, or different levels, whose incongruence is difficult to detect and handle. And this process may repeat itself, enlarging each time. (p. 378)

He regarded this pattern as progressive (p. 379) and cumulative, and responsible for the pervasive quality of binding among the family members. Obviously, the number of individuals implied in such a formulation goes beyond the dyad and Weakland in 1960 elaborated the DB formulation for three-party party systems and institutional relationships (e.g., administrator-therapist-patient or doctor-nurse-patient).⁹

⁹Weakland's article also attempted to relate observations from other groups investigating schizophrenia to DB formulations, an important DM function. Clearly, from the number of issues dealt with by this 1960 article, it was one of the more important of the early project publications.

Application to the three-person system. The extension to a three-person (or a three-party) interaction was technically important. Although clearly a systems approach in formulation and explicitly based on a cybernetic model, the DB paradigm had thus far addressed only the most simple system--the dyad.

The most salient points in the three-party elaboration relate to the greater potential for double-binding to remain unnoticed as the injunctions and qualifications may be distributed among several people. Remembering that the child is more dependent on both parents than either parent individually,

It is less obvious at first--but especially striking when perceived--that, even those obscuring factors that would seem inherent in the two-person situation may easily have parallels or equivalents in the three-person situation. Comparison and confrontation of possibly contradictory messages from one sender are difficult because the message cannot readily be separated. It may be equally difficult with two senders because the messages are too much separated--by person, by time, by different style of phrasing. And they still may differ in level: "When the double bind is inflicted not by one individual but by two,...one parent may negate at a more abstract level the injunctions of the other" (1956)...But of course these meta-level indications of unity and these claims of agreement and identity of messages are independent of actual similarity or difference in two messages--i.e., they may be false. Thus, the three-person situation has possibilities for a "victim" to be faced with conflicting messages in ways that the inconsistency is most difficult to observe and comment on that are quite similar to the two-person case. (1960, pp. 379-380)

Efforts to Place the DB in Certain Relationships to Other Paradigms for Schizophrenia

The DB group appears to have realized the importance of their

formulation relatively early, and efforts ensued to place the DB in certain relationships with other formulations. Thus, the DB idea was extended to new phenomena, and the DB corpus was differentiated from other clinical formulations especially psychoanalysis. Also, increased attention to the therapeutic uses of the DB revealed that the mechanisms of change in DB family therapy were radically different from those of individual therapies (whether interpretational or relational).

Extensions of the paradigm to new phenomena. Again, Weakland's 1960 paper emerges as important in paradigmatic and DM terms. He explicitly presented the objective of his paper to be the placing of the DB formulation in relation to rival formulations for schizophrenia.

This paper...centers on applying the approach and insights thus developed to the analysis of three-party interaction and to the interrelating of observations reported by various other investigations of schizophrenia. We hope in this way to give further evidence for our previous findings, to clarify a basic schizophrenogenic pattern common to a variety of particular situations, and to promote a communicational orientation that has been somewhat foreign to orthodox psychiatry but that we have found to be most illuminating. (Weakland, 1960, p. 374; emphasis added)

Weakland's extension to the three-party system produced what can be seen as a main-line extension or development of the DB paradigm as it elaborated aspects already implicit in the early paradigm in the same direction as the 1956 presentation. His subsequent extension to the three-party institutional situation constituted a tangent, or parallel, extension, as institutional dynamics were neither implied nor of a central concern in the paradigm or DM. Weakland concluded that his

extension had proved helpful, and suggested further extensions to four-party systems in schizophrenia,¹⁰ as well as the affective disorders and also for communication in institutions. (1960, p. 387)

The DB paradigm was extended to a wide variety of psychiatric disorders other than its original problem, schizophrenia. Watzlawick (1963) documented that the DB was also extended to delinquency (Ferreira, 1960) and to delinquency and addictions (Coodley, 1961), and to different classes of phenomena including: psychiatric training (Appel, et al., 1961; Coser, 1960); humor (Fry, 1963); creativity (Bateson, 1956); and existentialism (Watts, 1958 and 1961). Finally, its universal role in all psychopathologies was criticized (Abeles, 1976, p. 121).

All of the above may be regarded as tangential, or side-line extensions of the paradigm, which produce parallel lines of work which may or may not remain part of the DM. Some parallel developments, e.g., Fry's work in humor, remained within DB strictures, and as such, remained a DM side development. To "split-off" from a DM, a parallel development

¹⁰"This would be relevant to many unresolved questions, such as the role of a sibling in a schizophrenic's family, or the nature of the interaction occurring when a potentially schizophrenic adolescent becomes involved in a love relationship outside the family."(p. 387) It's particularly interesting in paradigmatic terms, that Weakland refers to the DB as an "analysis of communication patterns," neglecting the homeostatic element, when during his three-person extension, the homeostatic element emerges so clearly. This is an example of the assumption by DB people that the formulation was preponderantly communicational; the relational elements appear to have been acknowledged only indirectly. Even Weakland, the most sensitive of the three remaining members to Jackson's relational view, fell into this pattern at times.

would have to be elaborated to such an extent that anomalies would arise and a new paradigm formulated. From this view then, a paradigm, in the course of elaboration, may eventually foster several lines of development, with a main-line and parallel research lines. If, in the course of normal science activity any of these lines of development were elaborated to the point where anomalies emerged, it would be possible for a paradigm to be developed for that particular line of development; then this line of development would be a DM in its own right. This would directly imply geometrical progression of DMs rather than linear. Perhaps one way of judging a paradigm's importance relates to the number of main-line and side-line DMs it engenders.

The extension of the paradigm to therapy, especially Haley's work, constituted a main-line extension as Haley developed ideas important in the paradigm statement and stayed directly within DM concerns. (A line of development about humor, for instance, appears less central to DB concern, though it partakes of DB paradigmatic formulation regarding levels of communication). The original paradigmatic statement addressed the "nature, etiology and therapy of schizophrenia" but inspection of the 1956 publication indicates relatively little treatment of therapy. (In Chapter VI, it will become obvious that the structural and etiological features of schizophrenia were the groups initial interests; therapy, though included in the paradigm statement was explicitly developed only later.

Extension to therapy, with a new mechanism of change. Within two years of their paradigmatic statement, the group had begun explicit extension of the DB to therapy. In 1958 Weakland and Jackson broached the idea of the therapeutic DB, and Haley re-interpreted psychoanalytic therapy in communicational terms. The following year, Haley addressed the idea of control in psychoanalytic therapy and Jackson the practice of conjoint family therapy with regard to homeostasis.

During 1961, Haley extended his interest in control to brief therapy (1961a) and to psychotherapy with schizophrenics, (1961b), while Jackson joined Satir (Jackson and Satir, 1961) for family diagnosis and therapy, and Weakland (Jackson and Weakland, 1961) for the theory and techniques of conjoint family therapy. Bateson (1961) considered research into psychotherapy. Subsequently, Haley (1962) considered the future of family therapy, then with Jackson (Haley and Jackson, 1963) reinterpreted transference in DB terms.

Haley, especially, has continued to write about DB family therapy (Haley and Hoffman, 1967; Haley, 1976).

Their extension resulted in a form of therapy with different techniques and mechanisms of change, i.e., DB family therapy was revolutionary with regard to psychotherapy. First, there existed a consensus that conjoint family therapy was discontinuous with family-oriented therapy, or collaborative individual therapies.

One way of narrowing the definition of "family therapy" is to differentiate "family-oriented treatment" from "family treatment"... "Family-oriented treatment" usually

refers to those treatment approaches in which an individual family member is the primary focus of therapy, the consideration of other family members (usually parents or spouse) being secondary. In these approaches, the other family members usually are seen separately from the patient, and not infrequently by a different therapist...In the "family treatment" group of methodologies, three or more of the family members are seen simultaneously, i.e., "conjointly", by the same therapist or therapists. This author prefers to categorize these latter methodologies, when carried out as a continuous treatment, as "unit" family therapy. Families so treated usually include at least both parents and a specific child. (Freeman, 1964, p. 35)

Jackson and Satir (1961, p. 29) regarded conjoint family treatment as therapy in which "all family members are seen together at the same time by the same therapist." Family inclusion is based upon intimacy and association as well as biological kinship.

The inception of conjoint family therapy rests with the DB group. Grotjahn's work was obviously "family-oriented treatment," and Ackerman's family work was developed historically later, though independently. In fact, for Grotjahn, the individual was primary and the family secondary; for Ackerman, the family was primary with the individual secondary; for the DB paradigm, the family was primary and the individual ignored--in theory and technique, the DB approach was a purely systemic affair. The individuals assumed importance only insofar as they collectively constituted the system, and not with respect to the integration of individual and systems processes.

Also, conjoint family therapy had been differentiated from group therapy, the latter of which was regarded by Haley (1971b, pp. 1-2) as an "artificial collection of strangers." Family therapists worked

with people who had shared histories and usually shared futures.

To change these sorts of systems, it was necessary to reconceptualize therapy (Haley, 1971b, p. 3), decreasing the emphasis on personality, and appreciating the Gestalt formed by the families as a system (Jackson, 1957a, p. 181). With this change of focus, a different mechanism of change developed.

By the end of the 1950's it was becoming clearer that family therapy was a different concept of change, rather than merely an additional method of treatment to be added to individual and group therapy. The focus of family treatment was no longer on changing an individual's perception, his affect, or his behavior, but on changing the structure of a family and the sequences of behavior among a group of intimates. With this shift, it became clearer that neither traditional individual therapy nor group therapy with artificial groups was relevant to the goals and techniques of family therapists. The problem was to change the living situation of a person, not to pluck him out of that situation and try to change him. (Haley, 1971b, p. 4)

The mechanism of change in DB therapy was clearly discontinuous from those of either the traditional psychoanalytic therapies, or the later relational ones.

The mystery of what causes change has been only slightly clarified. Many new therapists beginning to treat families could abandon the idea that transference interpretations or insight into unconscious processes cause change. (Often they abandoned these concepts without realizing that this could also mean abandoning the theory of repression.) But as former individual therapists began treating families, they learned that some experienced family therapists were even doubting that helping family members understand how they deal with each other is related to change. It is beginning to be argued by many family therapists that talking to family members about understanding each other is necessary because something must be talked about and families expect this form of discussion, but that

change really comes about through interactional processes set off when a therapist intervenes actively and directly in particular ways in a family system, and quite independently of the awareness of the participants about how they have been behaving. (Haley, 1971b, p. 7)

The goals of changing the family structure and sequences of behavior demanded techniques significantly different than had been previously needed. Interpretations or reality relationships availed the therapist nothing, except perhaps inclusion in the schizophrenic family processes. Recalling that early DB revisions addressed the logical unavoidability of mutual binding and binding spirals, it is obvious that rational, linear attempts at intervention or explanation would compound rather than ameliorate double binding. In a schizophrenic family, any communication no matter how straightforward, is subject to an active binding process by the receiver. By implication, the only therapeutic intervention is a double bind in kind. If non-paradoxical communication is made double binding, and if all straightforward efforts are subject to inclusion in the family schizophrenic pattern, only a perverse, non-rational, already binding injunction can avoid corruption into binding.

The natural development in technique was the therapeutic double bind, broached by Weakland and Jackson in 1958 (p. 120)

We feel that the similarity of circumstances between original significant events and their re-creation, review, and reworking in therapy is crucial, and is not confined to the session presented here. Especially, ... we believe that something resembling the "double bind" must often be instituted on the patient by the therapist to obtain therapeutic change. This "therapeutic bind,"

however, must also differ in such a way as to require not the distortion, denial, or unawareness of the nature of vital interpersonal relationships of the patient but, rather, increased awareness of their true nature.¹¹

The formal structure of therapeutic and schizophrenic (or malevolent) double binds are similar, but the pathogenicity of the latter are due to their role in deuterio-learning, or learning sets. That is, the individual learns to respond only within the paradoxical and disorganizing framework of the double bind, and thus learns to trap him/herself. As Abeles (1976, p. 122) points out, paradoxes and double binds abound in life; response to them becomes pathological only when the individual remains trapped by futile attempts to respond or unravel them from within.

A second difference between therapeutic and malevolent double binds is that in the former, at least one person recognizes the situation, "indeed intentionally (if intuitively), sets it up in order that the patient shall escape it" (Abeles, 1978, p. 122). With the situation recognized, at least one person has benign control over the spirals; it is for this reason that the group continually stressed the importance of denial or concealment in malevolent binds.

Translated into techniques, the therapeutic double bind could take

¹¹Weakland and Jackson would apparently consider the relational aspect; Haley explicitly eschewed it. Bateson's position appears to me less clear, perhaps because his role was more as an intellectual, conceptual leader than as a therapist. Almost none of his work dealt with doing therapy.

the form, for instance, of prescribing the symptom. (This is probably the best known paradoxical strategy.)

Paradigmatic of this approach is the practice of "prescribing the symptom," i.e., telling the patient to continue behaving as he already behaves, with specific instructions to continue the symptomatic behavior. The paradox lies in the patient's inability to do so and still maintain the usual claim that the symptom is out of his or her own control. Any symptomatic behavior occurring now occurs within this new frame; it may thus be defined as following doctor's orders. The implication of following orders is that the patient might choose to not follow the orders; if so symptomatic behavior becomes, paradoxically, chosen, and by implication under the patient's control. The patient can gain release from this bind by refusing to cooperate with such a peculiar prescription--in which case he does then exercise control--and is free of the symptoms. (Abeles, 1976, p. 123)

Interestingly enough, the technique works, and often the patient exhibits a thwarted, "I know you're doing something but I don't know what and it's foolish anyway."

Another set of techniques lies in exposing covert conflicts that are verbally covered-over by the double bind patterns. Typically, in family sessions early in therapy, the discussions revolve around the identified schizophrenic patient; it is important to deflect talk away from the patient's condition and toward the anxiety laden areas of covert conflict. In one family,

the father and mother insisted for some time both that they were in agreement on all important matters and that everything was all right in their family--except, of course, the concern and worries caused by their son's schizophrenia. At this time he was almost mute, except for mumbling, "I dunno" when asked questions. During several months of weekly family interviews, the therapist tried to get the parents to speak up more openly about some matters that were obviously

family problems, such as the mother's heavy drinking. Both parents denied at some length that this was any problem. At last the father reversed himself and spoke out with only partially disguised anger, accusing his wife of drinking so much every afternoon with friends that she offered no companionship to him in the evenings. She retaliated rather harshly, accusing him both of dominating and of neglecting her, but in the course of this accusation she expressed some of her own feelings much more openly and also spoke out on differences between them. This session was reviewed and discussed with the participants the next week (and a tape recording of the argument was played back). In the following session, the son began to talk fairly coherently and at some length about his desires to get out of the hospital and get a job and thereafter he continued to improve markedly. (Weakland, 1960, p. 383)

What is technically relevant here is that "mere exposure of covert parental conflicts, even before they are resolved, is accompanied by patient improvement" (Weakland, 1960, p. 386; emphasis added). By shifting the verbal processes, the homeostatic aspects of the family are jarred, the mother begins to appear more disturbed, father more absent, and son less crazy, as his pathology is no longer "needed" homeostatically to deal with mother's drinking and father elopement. At no point are insights gleaned, or understanding shared, or relational elements recognized. The DB theorists suggest that mother is exposed as alcoholic and father as neglecting--rather harsh and seemingly irrelevant to the patient's condition. Yet, by shifting the focus from the schizophrenic to the covert disagreement and engendering open conflict, the patient can systematically improve. Thus, the focus, and mechanism of change, differed radically from previous therapies. The family as a unit was seen and the Gestalt of interaction became the

focus. Mechanisms of change revolved around changing the structure and the sequential behavior patterns of the family. With the revolutionary status of its clinical practice as well as theoretical formulations regarding the nature and etiology of schizophrenia, the DB paradigm began the fourth major class of psychotherapy, after interpretational psychoanalysis, relational psychotherapy and group therapy.

Differentiation from Psychoanalysis

Relatively early in paradigm elaboration, the DB group attempted to define its formulation's role vis-a-vis other formulations, with respect to both theory and practice.

First, the group explicitly eschewed any organic or hereditary formulation of schizophrenia, on the grounds that such an approach had produced little (Bateson, et al., 1956) and it unnecessarily limited available data (Weakland and Jackson, 1958). Also, a position was taken that double bind family therapy necessitated a decided conceptual shift, as it was discontinuous with the clinical work prior in time to it. For instance, Haley (1971b, p. 9) stated that recent visits to university teaching hospitals (he discounted state hospitals here) "illustrates how discontinuous the family, or ecological, view is from the psychiatric orientation still being perpetrated in the better universities." Haley regarded the usual recommended course of treatment for schizophrenia (long-term hospitalization and individual psychoanalytically oriented therapy) as "absolutely contraindicated." Such an approach,

he felt, was

based on an ideology which assumes that the patient's problem is internal and his social situation is secondary. The family view adopts quite the opposite view--the [patient's] problem is his social situation and his internal dynamics are a response to that situation. (Haley, 1971b, p. 10)

This was clearly a radical inversion of traditional etiological formulations, with special reference to psychoanalytic work. Jackson and Weakland (1961, p. 17) made the same type of differentiation.

In brief, we are much more concerned with influence, interaction, and interrelation between people, immediately observable in the present, than with individual, internal, imaginary, and infantile matters. It is worth making this difference in basic orientation explicit, since to do so helps clarify the nature of our main specific concepts, indicates some important connections between them, and provides a background essential for understanding our whole therapeutic approach--what we do and what we do not do, especially some of our differences from other therapeutic concepts and practices.

The differentiation from psychoanalytic work occurred in specific areas of theory, as well as general orientation. For instance, in 1957(a) Jackson addressed the etiology of schizophrenia, assigning primary etiological significance to continuing or repetitive events which establish a condition of life for a patient-to-be, rather than the psychoanalytic concept regarding the role of trauma or psychological traumatic assault (see pp. 182 and 193). Two years later Haley (1959a) differentiated the systems with regard to intra-psychic vs interpersonal processes.

Despite all that is said about difficulties in interpersonal relations, psychiatric literature does not offer a systematic way of describing the interpersonal behavior of the schizophrenic so as to differentiate

that behavior from the normal. The schizophrenic's internal processes are often described in terms of ego weakness, primitive logic, or dissociated thinking, but his interpersonal behavior is usually presented in the form of anecdotes. This paper will present a system for describing schizophrenic interactions with other persons, a system that is of necessity based on a theoretical framework describing all interpersonal relationships. (Haley, 1959, p. 321)

Haley later (1971b, p. 2) addressed differences with respect to therapeutic isolation or social integration and the intra-psychic vs interpersonal element, as well as mechanism of change--clearly perceiving "clinical change as resulting from changed relationship rather than insight."¹²

An essential part of the medical model was the ideal that a person could be changed if he were plucked out of his social situation and treated individually in a private office or inside a hospital. Once changed, he would return to his social milieu transformed because he had been "cleared" of the intrapsychic problems causing his difficulties. In this model, primary change was effected by providing the "patient" with insight into his unconscious conflicts, thus eliminating the repressive forces which were incapacitating him. The real world of the patient was considered secondary since what was important was his perception of it, his affect, his attitudes, the objects he had introjected, and the conflicts within him programmed by the past. While a science of human behavior was being conceptualized in social terms under the influence of systems theory, the people who were trying to change people were determinedly disregarding the social environment. (Haley, 1971a, p. 2)

Jackson and Satir (1961, p. 43) clearly differentiated family work

¹² It should be kept in mind that for the DB project members, especially Haley, changes in relationships followed structural and behavior sequence changes, not understanding or insight.

from psychoanalytic with respect to differences in clinical practice and reception of their work.

We feel that just as events point to increasing union between psychiatry, the family, and social science, there will be no such union in the main current of psychoanalysis for some time to come. Although there is a small group of psychoanalysts who are interested in participating in family studies and research, there is a much larger group who do not consider this work immediately relevant to their own interests, and even a rather hardbitten group who feel that current family approaches are superficial and tangential and can in no way be compared scientifically with the depth analysis of psychoanalytic therapy. There is also a group of well meaning psychoanalysts who are attempting to correlate and collate family data with their own observations as individuals, but who unwittingly do the family movement a disservice. This is because some of them feel that knowledge about family individuals is old stuff and is now merely being refurbished. Their descriptions of family work are largely couched in the monadic framework of psychoanalytic terminology and are still essentially individual. They have not yet become convinced that the parts are greater than the whole; their main tenet is that the treatment of a family is theoretically impractical because of the difficulty the therapist has in handling more than one transference at the same time. This latter observation is part of the reason why family diagnosis and therapy needs a new terminology since the concept of transference.

There are a number of interesting points in their attempt to differentiate themselves from other formulations, and especially psychoanalysis which they single out. During this time, there were a number of competing individual therapists, yet the double bind paradigm was differentiated with respect to them once or twice and the matter was settled.

This repeated concern with psychoanalysis could be interpreted as

the result of its pre-eminence, and there are of course such elements present. But if one recalls that to be revolutionary, a paradigm must be revolutionary for a particular group, it can be inferred from these efforts, that the DB paradigm was revolutionary for the interpretationist psychoanalysts, and not for the relational or group psychotherapists. Two historical trends support this contention. First, the major anomalies which the DB paradigm dealt with arose from within the interpretationist efforts and extensions, and not the relationists. Secondly, the DB paradigm "solved" the technical problems of treating schizophrenics and latent schizophrenics that had plagued the interpretationists, but not the relationists. Thus, though the relational therapists were temporally closer to the DB period, the DB was revolutionary for interpretationist psychoanalysis.

The DB members attempted to maintain control of their paradigm in a number of ways--by correcting misunderstandings of definitional or conceptual paradigmatic elements and by emphasizing the impermeable barrier between psychoanalysis and DB family work. They also attempted to incorporate other formulations, by such cognitive processes as reinterpreting data or concepts from other theories into DB terms. Jackson (1957a, p. 183) used the work of other researchers to corroborate a point regarding codification of perception and identity, but this is different than the reinterpretation mentioned. Weakland's important 1960 paper does the latter, at times explicitly.

Still it may be noted that empirical evidence is already accumulating to demonstrate that the possibilities suggested above are actually encountered in families with a schizophrenic member and are relevant to the schizophrenia. For example, although the work of Lidz and his coworkers is oriented and conceptualized quite differently from our own, many of his observations on family interaction fit in with our schema. Typically, the families studied exhibited either "marital skew", or "marital schism". The "skew" situation is one in which the father and mother apparently share some rather peculiar view of marriage and family life...These excerpts make it clear that Lidz's "family skew" involves situations of apparent parental agreement but covert disagreement, situations which, in terms of communication, must involve incongruent messages to the children, but with concealment, denial, and inhibition of comment operating in the ways we have outlined. Indeed, one case description approaches these terms. 'Although neither parent overtly discredited the other to the children' yet within the family there were 'obvious' or 'apparent' indications about father, mother, and their relationship which resulted in 'perplexing discrepancies' and 'inconsistent and contradictory images.' (Weakland, 1960, p. 381)

...Lidz's "marital schism" category is not so easy to align with our views, because it involves and indeed emphasizes open parental disagreement, "severe chronic disequilibrium and discord". Nevertheless, it seems that these families might, in broad terms, be seen as being in line with our schema. (Weakland, 1960, pp. 380-381)

Weakland went on to also incorporate some of Bowen's (1959) work with respect to the formalized ways in which families avoid open conflict in the face of emotional distance (p. 382) and Wynne et al.'s (1959) work on "pseudomutuality" or the concealment of conflicts (p. 382)

The re-interpretation of data and concepts can be seen as serving a number of functions: extending the boundaries of DB applicability; subsuming other formulations under the DB with the implication that other formulations are special cases of the more general DB; jockeying

for a place in the sun by addressing others who have already achieved some reputation and therefore making oneself known with respect to them; establishing the DB boundary as applicable within some other formulation; testing the limits of the paradigm's explanatory powers; establishing links with other frameworks to facilitate exchange of information. Probably each of these functions has been exercised at some point in time. The point, however, is that re-interpretation of other formulations into DB terms was a feature of DB paradigmatic elaboration and, may probably occur in the development of other paradigm-DMs.

A variant of this process also developed; terms and concepts from psychoanalysis were re-interpreted in DB terms. For instance, Jackson and Weakland (1961) evaluated the psychoanalytic concept of transference in DB terms.

Transference is a manifestation related to the inactivity prescribed for standard psychoanalytic treatment. The patient, on the basis of minimal cues, creates a framework and embroiders it with past personal references. In conjoint psychotherapy, there is a good deal of activity, even if the therapist is only acting as a traffic cop. If skillfully managed, the interaction is largely among family members and not with the therapist. Thus we would consider the proper intervention when a wife is chopping her husband to ribbons not to be "Look what you're doing to the poor man," but to ask him if she always shows her attachment to him in this way. The wife will be fascinated awaiting his reply and will be busy with her rebuttal.

That is, with so much interaction among the family members and active therapeutic focus on this, there is no emergence of standard transference phenomena. What we do see can better be labeled parataxic distortions, since the data consist of discrete examples of expectations on the part of a family member that the therapist does or does not fulfill... It is difficult to explain the difference between these

phenomena in individual and family therapy unless one has observed or participated in both forms of psychotherapy. A statement by a family member which, if it occurred in an individual psychotherapeutic session, may be labeled evidence of transference can have a very different meaning in family therapy. Thus, a comment by the wife that the therapist is the only one who has ever understood her is apt to be an expression of dissatisfaction with her husband, a pointing out of a direction he should take, and before the therapist can label this himself as father transference the husband's reaction will have to be dealt with, plus one of the children, plus the wife's reaction to her husband's reaction, and so on. (Jackson and Weakland, 1961, pp. 32-33)

Haley addressed psychoanalysis more generally (1958a), gave an interactional explanation of trance induction in hypnosis (1958b), and used his development of the idea of control in interpersonal relationships to analyze control in psychoanalytic psychotherapy (1961a) then with regard to the psychotherapy of schizophrenics (1961b).

Finally, the DB group differentiated itself from psychoanalysis by eschewing any phenomenological or experiential formulations or considerations, and insisting upon the importance of observable behavior and behavior patterns. For example, Jackson and Weakland (1961, pp. 16-17) stated with regard to the DB that the concepts are

...concerned with the description and specification of interaction among actual persons, by various means of communication, at a level of directly observable behavior. This focus implies further an emphasis on what is real and on what is current and continuing to occur. Taken together, these emphases define a broad "communicational" and transactional orientation to the study, understanding, and treatment of human behavior---including that special class most interesting to psychiatrists, symptomatic behavior. This orientation, while related to earlier work, especially Sullivan's, and currently increasing in acceptance, still is considerably

different from the strong traditional orientation of psychiatry emphasizing the individual patient and constructs about the unreal or unobservable: fantasies or misperceptions of reality; past, mainly childhood, experience; and intrapsychic organization and content. (Jackson and Weakland, 1961, pp. 16-16)

DB Family Therapy

Family therapy based on the DB differed drastically from psychotherapy as it had been done for several decades, and differed most sharply from psychoanalysis. In Haley's view (1971b, p. 10), traditional treatment was based on an ideology that assumed that the patient's problem was internal and his social situation secondary. "The family view adopts quite the opposite view--the [identified patient's] problem is his social situation and his internal dynamics are a response to that situation. These two points of view represent a discontinuous change in thinking about human problems and how to change them."

The resulting changes affected the unit that received treatment, the formulations, goals and techniques. The families of schizophrenics remained the primary focus of intervention efforts by the core group of authors. Such families were conceptualized as

enmeshed in a pathological but very strong homeostatic system of family interaction. That is, regardless of their past history--although that might be enlightening--they are at present, interacting in ways that are unsatisfying and painful to all, provocative of gross symptomatology in at least one, and yet powerfully self-reinforcing. Their overt behavior may appear varied or even chaotic, but beneath this a pervasive and persistent pattern can be discerned, and one that is quite resistant even to outside therapeutic efforts at change.

How and why is this so? On what basis may such homeostasis be clarified and understood? We may at least begin to do so by using further our basic concepts of the double bind and the still broader concept of the necessary multiplicity of messages, of different levels, in all communication. These ideas, which were helpful in understanding the occurrence of schizophrenic behavior, are also helpful in attacking the more fundamental problem level: why does pathological behavior or organization persist, even under pressure to change? (Jackson and Weakland, 1961, pp. 28-29)

This resistance to change, or alternatively, persistence of pattern, was attributed to the spiraling effects of mutual binding within the families; also, owing to the complexity of communication, it was possible to avoid both agreement and disagreement without its being noticed by "using" incongruent messages. In particular, the group focused on "disqualifiers"--messages that negate what another person has said, but in an indirect way, so that statements are not really met. (Jackson and Weakland, 1961, p. 29)

Family members were perceived as bound together in mutually destructive patterns, the primary symptom of which was apparent only in the patient. In therapy, there would commence persistent efforts to preserve a cohesive facade, usually by focusing on the "fact" that everything was fine, except for the patient's schizophrenia. Once the facade had cracked (due to any number of techniques, some of which will be discussed shortly), the covert disagreement, or split, or conflict, between two other family members became apparent. With its emergence,

the patient usually improved symptomatically,¹³ though naturally the family remained likely to embark on further double binds unless intervention continued (Jackson and Weakland, 1961, p. 19).

Obviously, because of these processes exemplifying double bind and homeostasis, the unit of focus was necessarily the family rather than the individual. This point was consistently emphasized by the DB workers, and acknowledged as central to DB family therapy by later adherents (Freeman, 1964, p. 36; Klein, 1963, p. 26). In fact, treatment of the family as a group or system was necessary to meet the form of the concepts used (e.g., homeostasis), and group or systems processes constituted the conceptual frame of reference. Haley (1971a, p. 231) even insisted that the therapist formed part of the system and was constituent in the diagnosis of that system.

Formal diagnoses were abandoned as they were based on individual psychology, could not be readily translated into DB terms, and contributed almost nothing to the change process (1971a, pp. 232-233). Haley in particular was adamant that clinical results were the hallmark of good clinical practice. "Unless the diagnosis indicates a program for bringing about change it is considered irrelevant by the more experienced therapist."

¹³ Interestingly enough, Jackson and Weakland later remark (p. 21) that extended contact with these families usually reveals not only that these parents usually have considerable interpersonal difficulties, but that these difficulties are very often similar to those the patient exhibits in his symptoms.

The focus of the change process, or the goal, was neither understanding nor abreaction, but changing of the "self-reinforcing and mutually destructive networks of interaction" in families (Jackson and Weakland, 1961, p. 26). Through the seeming disorganization or the profusion of symptomatology, the communicational and homeostatic patterns held the families in frozen positions; thus it was processes rather than symptoms that became the target of intervention attempts.

One of the things that the tyro therapist must experience is that he will have to deal with the same problem over and over again in different forms and guises, as the following example suggests.

Initially, the father of a paranoid patient complained to the therapist of his son's obesity and requested a diet for him. He and his wife expressed futility about "doing anything with him." They occasionally took action of an interesting sort, considering their son's suspicious nature; for example, the father sneaked out early one morning to tell the milkman that he was to ignore any requests for ice cream. The therapist held fast to his recommendation that the patient would change himself when he was ready, and several sessions later the patient announced that he had lost some weight. As the therapist tried to congratulate him, the mother cut in to discuss her own weight problem, and the father topped her by recounting a rather bizarre episode in which he was found unconscious and taken to a hospital in peril of his life.

This sequence was characteristic for this family. The patient's statements tended to be ignored or rationalized away, the mother usually sounded a serious note about something, and the father topped it by telling something on himself which, while dramatic, inevitably made him out to be slightly foolish. A kind of closure was usually attained at the end of these sequences by the father, mother, and son all chuckling slightly at the father's expense. This sort of closed sequence, however, constitutes the sort of pathological family homeostasis that it is the therapist's business and duty to alter. (Jackson and Weakland, 1961, pp. 25-26)

In the attempt to induce change, DB family therapy ignored intra-psychic dynamics for more conscious levels of family interaction (Klein, 1963, p. 26; Gralnick, 1962, p. 519), eschewed content for communicational and structural concerns (Jackson and Weakland, 1961, p. 26), and concentrated on what maintained the form of the current structures rather than historical concerns (Gralnick, 1962, p. 519; Haley, 1971a, p. 230).

To make these sorts of changes, the therapist had to be quite active in the sessions¹⁴ (Jackson and Weakland, 1961, p. 25; Gralnick, 1962, p. 519; Freeman, 1964, pp. 37-38; Haley, 1971a, pp. 230-231), often making interventions in the first session, before full information was gathered, so that change processes were started while the family is still in crisis and most amenable to change (Haley, 1971a, pp. 230-231). (This strategy is also very helpful in establishing some control over the situation for the therapist and in getting some form of change, which

¹⁴ "From the discussion so far it must now be evident that active intervention in and management of family interaction has an important place in our initial work, and, indeed, this holds true of the further course of family therapy also. This active orientation, however, grew out of our experience and was not a predisposition except that experience in treating individual schizophrenics presses one toward an active and varied style of therapy. Nevertheless, in beginning our work with families, we were concerned lest activity on the part of the therapist would obscure family operations and dim the light of our research. Actually, it has been so difficult to keep the sicker families involved, to produce shifts and not mere repetitions of the standard patterns characteristic of any one family, that we are no longer so concerned about the therapist remaining a flyspeck by his own design and efforts, and more concerned with his avoiding being put into such a useless position by the family." (Jackson and Weakland, 1961, p. 25)

of course increases the therapist's credibility and therefore his/her chances of success).

To do this, the DB therapist might "frame the therapy" as a whole, that is, set up, often implicitly, broad expectations, rules and guidelines for the family (Jackson and Weakland, 1961, pp. 22-23). For instance, s/he might interrupt the parents' attempt to focus solely on the identified patient's symptomatology or history (Jackson and Weakland, 1961, p. 24); s/he may re-frame messages within the family so their meaning shifts (e.g., from Laing's "bad" to "mad" or vice-versa). This experience demonstrated that pointing out double binds or spirals did little good; however, the meaning, intent or focus could be shifted by such re-framing, and with repetitions of such shifts, the pattern was said to lose some of its "highly stereotyped repetitiousness." (1961, p. 26). Advice was sometimes given, to accept the help offered, rather than to do the "right thing."

At times, specific instructions were given, usually with regard to a minor matter, but one which involved a significant pattern or interaction "and given an instruction to do A, expecting that the person, from our knowledge of his reactions, will in fact do B, which will cause change C in a family relationship." (Jackson and Weakland, 1961, pp. 26-28) Clearly, however, this form of instruction was not the straightforward matter usually envisaged by the term. It partook of the "therapeutic DB," the benign use of double binds by the therapist to gain change. This technique was especially useful for schizophrenic

families as it matched their own form of communicating, enabled the therapist to avoid entrapment in the family's communicational pattern, confused the usual homeostatic processes, and necessarily induced change (as a therapeutic double bind is as impossible as a family double bind to ignore, obey or not obey).

In other words, these families have a tremendous aptitude for "plus ca change, plus c'est la même chose." It appears increasingly clear to us as we work with them that to be effective we must meet them on their own ground, though with different orientation--toward positive change instead of defensive maintenance of a sick system. That is, the therapist must himself employ dual or multiple messages involving such incongruences as will serve to come to grips with the whole complexity of the messages of the family members he must deal with...That is, we have been concerned with using explicit statements that convey concealed and unexpected implicit meanings as well, with using content messages joined with framing statements, with giving instructions whose carrying out will constitute a further message. We have spoken of this elsewhere, perhaps too narrowly as the "therapeutic double bind"; the broad principle described here, of using multiple--and often incongruent--messages therapeutically, is what needs recognition, and then further investigation. (Jackson and Weakland, 1961, p. 30)

Collusion in factional struggles was avoided, except as a temporary, strategic, and explicit maneuver (Haley, 1971a, pp. 234-235). Implicit collusion, particularly if denied, with some individual member was usually an indication of poor prognosis.

Messages were interpreted and double binds uncovered or exposed (Klein, 1965, p. 26). Feelings or attitudes, however, were most decidedly not interpreted, and interpretation as usually construed was regarded as actively destructive in this form of treatment.

The beginning family therapist tends to feel that it is

helpful to the family to bring out their underlying feelings and attitudes no matter how destructive these might be. He interprets to family members how they are responding to each other and expressing their hostility through body movement; and so on. Often he feels this is a way of giving meaning to the family members. The more experienced family therapist has less enthusiasm for the idea that interpreting feelings and attitudes brings about change. In particular, he does not feel it is helpful to confront family members with how much they hate one another. Instead, he tends to interpret destructive behavior in some positive way, for example, as a protective act. His premise is that the problem is not to make explicit underlying hostility but to resolve the difficulties in the relationships which are causing the hostility. Therefore the more experienced therapist is sparser with interpretations except when using them tactically to persuade family members to behave differently. At times the beginner may seem to be torturing a family by forcing them to concede their unsavory feelings about each other. The more experienced therapist feels this is a waste of time and not therapeutic...Although more experienced family therapists do not emphasize negative aspects of family living, they are quite willing to bring out conflicts if doing so is necessary to break up a particular pattern. (Haley, 1971a, p. 233; his emphasis)

Double Bind Paradigm as Revolutionary

To be a revolutionary paradigm, a scientific development must meet certain criteria; it must be: 1) a problem solution that is 2) a new way of seeing things (a reconceptualization), that 3) has an analogue function, (i.e., it serves as a Gestalt with which to "see" new problems as subjects for the application of similar forms of conceptualization and techniques). This solution/analogue must be 4) sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity (i.e., establish its "own" DB),

5) for a time provide model problems and solutions, yet 6) be sufficiently open-ended to leave problems the new, re-defined group can address and, 7) provide the nexus around which group commitments can be developed, and thus, provide some criteria by which to recognize adherents to one's "own" group and to other "competing" groups with their own paradigm.

Moreover, a revolutionary paradigm would bear certain characteristics. Such a paradigm would be constituted A) from identified anomalies, which have developed into B) a spirit of crisis (i.e., awareness of fundamental difficulties with a framework); the paradigm would be formulated C) deductively, in the course of D) normal science efforts directed at other goals, by E) people with very different perspectives outside the framework in crisis (i.e., Masterman's "rank outsiders"), which results in a solution set that is fresh, but F) relatively crude.

Most of the criteria and supplementary characteristics for a revolutionary paradigm have been demonstrated here to be relevant to the DB formulations.¹⁵

The DB paradigm directly addressed a number of obdurate anomalies in classical psychoanalysis. In the traditional clinical population, comprised of adult transference neuroses, the anomalies included:

¹⁵ Several such criteria and characteristics have not yet been reviewed, including the deductive nature of the paradigm, its relative crudity, and its development by "outsiders" during the course of normal science activity in pursuit of other goals.

the presence of neurotic complementarities in dyads and the persistently poor social relation in many "successfully" analyzed individual patients, and paradoxical reciprocities in health and disorder in the families of some patients whose clinical conditions were improving. These occurrences highlighted the importance of contemporaneous interpersonal factors, which neither the classical change process, nor theory, could accomodate.

Among the new clinical populations to which psychoanalysis was extended, the psychoanalysis of children provoked several anomalies with respect to mechanisms of change and technique. Isolation of the treatment from family members was required to foster the transference, without which there could be no working through and thus no change; however, when working with children, contact with parents was necessary if for no other reason than to enlist their cooperation, and precluded their removing the child from analysis. Thus, technically, the analyst was caught in a bind; s/he could see the parents and hamper transference, or not see the parents and endanger the treatment. Moreover, the child apparently did not form transferences of the classical type anyway, and therefore new techniques and mechanisms of change were required.

Finally, several anomalies emerged in the psychoanalysis of schizophrenics and borderlines. With schizophrenic patients, the anomalies included: presence of insight and affect but little behavioral change and thus very low "cure" rates; reciprocities of health and disorder among family members as the schizophrenic member's condition changed; change in schizophrenic condition with contemporaneous change in a

family member; a high relapse rate; and a lack of success with schizophrenic communication. Borderline anomalies included: the formation of transferences despite a markedly narcissistic clinical picture; the precipitation of psychoses rather than cure during analysis; and the lack of change despite insight and affect.

The DB proposed a problem solution to these anomalies in which the Bateson ideas on levels of communication and paradox addressed the difficulties of schizophrenic communication and Jackson's homeostatic ideas, the complementarity, reciprocity and relapse anomalies. Less directly, the DB was a solution to precipitation of psychoses. (See Jackson and Weakland, 1958).

The problem solution constituted a new way of seeing things, essentially a reconceptualization of schizophrenia as a psychological, rather than organic disorder (Bateson, 1956; Jackson, 1957a, p. 181; Haley, 1959a, pp. 322-323), and as behavior that was organized and learned from an interpersonal context where such behavior was both meaningful and appropriate (Abeles, 1976, p. 113).⁶ The new viewpoint emphasized the interpersonal aspect rather than intrapsychic and held the opinion that the etiological factors were nondiscrete and continuing rather than a circumscribed trauma.

¹⁶As Abeles (1976, p. 117) later points out, the basic assumption of the social intelligibility of symptoms in schizophrenia is shared among several other formulations; she included those of Laing; Searles; and Wynne, Ryckoff, Day and Hirsch.

The DB served the analogue function for other psychoses, the neuroses (!), delinquency and the human condition. It was sufficiently unprecedented to attract its group of adherents, especially among those working with schizophrenia, or new to clinical work and as yet uncommitted to a particular approach.

Model problems and solutions emerged as the DB paradigm was elaborated. Several such problems included: developing the homeostatic aspect to the same level as the communicational, controlling and developing the boundaries of definitions for important concepts, shifting from "binder-victim" to mutual-binding, moving to the more-than-two-person system, developing the spiraling concept to account for the pervasiveness of double binding in a family, elucidating precipitants, developing a change mechanism for family therapy, and setting the DB in some relationship with other formulations.

Finally the DB provided the nexus about which group commitments and identity could be developed. The formulation was clear, explicit, and radical enough that a clinician could not accept it by default or slide into it; one had to adopt it from whole cloth, or not at all. Since the DB provided a formulation, change agent techniques and theory, it was virtually impossible to re-interpret into any other formulation, as any component so interpreted would bring in by association other elements, until the whole system was in evidence.

Finally, the paradigm emerged during a period in which there was an awareness of crisis and controversy regarding fundamentals of the classical psychoanalytic framework.

CHAPTER VI

THE DOUBLE BIND DISCIPLINARY MATRIX

In 1951, Gregory Bateson and Jergen Ruesch published Communication: the Social Matrix of Psychiatry in which they used Russell's theory of Logical Types to analyze communication.¹ Among the insights generated was the idea of dual levels of communication; each "unit" of communication was perceived as having both a report and a command aspect. The report aspect conveyed information ("Nice day, isn't it?"), while the command aspect defined or limits the conversation (implicitly - "We'll talk about the weather now."). This dual structure of language formed the foundation of the DB paradigm and project begun by Bateson one year later. (Clearly, the report aspect is analogous to the idea of meta-communication).²

The inception of the DB DM, particularly as it relates to the development of the communicational and relational aspects of the DB paradigm will be the present focus. The DB DM had a formal life span of ten years (1952-1962) and was enormously successful in terms of originality and productivity;³ the project was established, funded, and

¹Bateson is customarily credited with this particular development.

²The command function was later developed by Haley in his work on control over the definition of relations, to be reviewed shortly.

³Haley (1976a, p. 110) credits the group with more than 70 papers. (He is including apparently Bateson, Weakland and himself as "group members" and Jackson and William Fry as "consultants.")

functioned as a research group for various facets of communication, all based upon the Russellian levels. The project also satisfied the formal criteria for DM status: 1) a coherent research tradition; 2) a constellation of group commitments ("conceptual, theoretical, and methodological") which define a scientific community; and, more implicitly, 3) a set of "scientific habits" (including intellectual, verbal, mechanical, and/or technological).

A DM also has an internal structure comprised of at least one (or more) paradigm(s), symbolic generalizations, values, and heuristic and/or metaphysical models. Finally, it serves several functions in the conduct of scientific activity; a DM provides structures and processes by which paradigms can be articulated with relative unanimity of problem-choice, adequate solution, and communication. Essentially a DM provides a "sphere of facilitation" in the conduct of scientific activity, as well as a framework of scientific accountability. All of these functions or characteristics were fulfilled by the DB project.

In this chapter, the focus will be on the inception of the DM, particularly as expressed in the group's formation around a developing paradigm, and the DM's subsequent characteristics and development. The time span considered will be the decade of the group's formal existence, that is 1952-1962. Although the members continued to produce papers after the 1962 dissolution, they did so as individuals or occasionally pairs, but on clearly diverging pathways, and not as a coherent research

group; moreover, not all their work remained consistently on DB issues.⁴

Inception of the Communicational Research Project

In 1952 Gregory Bateson received a grant from the Rockefeller Foundation to study the general nature of communication in levels (based upon the previously mentioned Russellian Theory of Logical Types). Previous to this, Bateson had studied human and animal communications and most recently communication among otters. Jay Haley and John Weakland joined Bateson early in 1953. The group was organized and funded as a research group into the nature of communication and in fact Haley (1972, p. 114) comments, "It was not a clinical project and was housed in the Veterans Administration Hospital only because Bateson was ethnologist there."

This point meets one of the supplementary characteristics for a paradigm as yet not met (i.e., the development of the paradigm arising from normal science efforts directed at different goals). By 1963 the group was quite aware that its work was often seen as exclusively or most importantly the DB, while they (and especially Bateson) reiterated that the focus of efforts was toward communication in its various aspects.

⁴At many points, Haley's 1976a account of the history of the DB group has proved invaluable; Bateson's comments upon it, the DB papers during and after dissolution, and the family therapy literature in general, has provided the information for this section. Unless otherwise noted, I am relying on Haley's 1976a history.

...although our investigations thus involved various fields of phenomena, and the particular concept of the double bind was a striking one--as attested by the specific attention that both we and others have given it--neither these specific subject-matters nor this specific concept has been the real core of our work as we see it. This point needs special attention, as it seems that a number of existing criticisms or misunderstandings of our statements rests on a lack of clarity at just this level..What is more important in our work, and may not have been sufficiently emphasized or clear in our 1956 paper, is a general communicational approach to the study of a wide range of human (and some animal) behavior, including schizophrenia as one major case. The present and future status of the more specific double bind concept can appropriately be considered only within this, its more general and inclusive framework. (Bateson, et al., 1963, p. 155; emphasis added)

Prior to the 1956 publication, the group had examined "the nature of metaphor, humor, popular films, ventriloquism, training of guide dogs for the blind, the nature of play, animal behavior, the formal nature of psychotherapy, and the communication behavior of individual schizophrenics." (Bateson, et al., 1963, p. 154)

Haley and Weakland joined Bateson early in 1953. Haley had previously been working on the social and psychological analysis of fantasy, particularly as seen in popular films. Weakland, though earlier trained as a chemical engineer, had more recently been working in cultural anthropology with emphasis on China. Later in that same year, Dr. William Fry joined the project as a part-time consultant, immediately upon finishing his psychiatric residency (Haley, 1976a,p. 61). In a particularly nasty trick of fate, Fry was summoned by the Navy for a one and a half year tour of duty in 1955-1956 and was not a co-author on the paradigmatic 1956 article. Haley says about Fry, "Had he not

been gone that year he would have joined the rest of us in writing that article." (1976a, p. 108)⁵ (Jackson joined the group early in 1954 and was a co-author).

With their backgrounds and early training, Bateson, Haley and Weakland obviously qualify as Masterman's "rank outsiders" with respect to schizophrenia, a clinical phenomenon which of course meets another supplementary paradigmatic criterion.

The first year of the group's collaboration was spent primarily in trying to find what Haley characterizes as "a common approach," (1976a, p. 61) and Bateson as "an adequate language" (Bateson, 1976, p. 108). The first difficulty that confronted them was whether levels of communication and paradoxes were "relevant to anything important in human life" (Haley, 1976a, p. 61). In particular, the question of truth and untruth became controversial; to logicians, truth in reality was often irrelevant; in human interaction, that was rarely the case. This point became increasingly important; Haley in particular, adopted a "value-free" stance in psychotherapy which appears related to this issue. For instance, using Laing's progression of "from bad to mad", a DB technique of reframing would shift a family's perception of the identified patient's behavior from "mad" to "bad" or vice versa. The truth of

⁵One wonders in fact, why he was not so included among the authors. If he'd been part of the project from 1953 and left in 1955, and if the article were written in 1955 (as it must have been), it is apparent that he'd had at least indirect influence on the paper, particularly during the crucial early years.

of the characteristics was essentially irrelevant; the reframing and shifting of the family's behavior was primary and constituted the goal of that intervention.

Development of the Communicational Aspect of the Paradigm

1953. As a common language began to develop, a set of virtual synonyms developed (Logical Type, Levels of Abstraction, Levels of Communication, Message and Metamessage, and Metacommunicative Level) which addressed the relationship between a message and the message which qualified it.

A beginning development in terminology occurred with the idea that one message 'frames' another message...it became possible to see a relevance between a paradox such as 'All statements within this frame are untrue,' and a piece of human communication where one person indicates with a framing message how his subsequent message is to be received. (Haley, 1976a, p. 62)

Obviously, with this development, the project had found or constructed, relevance for human affairs in the paradox's reflexivity. Bateson used film excerpts of otters to see if they qualified their messages. (They did, e.g., "This is play.")

Haley states that the work of that first year was "diverse" (1976a, p. 62). It is apparent that, in Kuhnian terms, the project did not yet have a coherent research program. They investigated: 1) otters playing (a new interest in line with the project's future developmental direction; 2) an analysis of a popular moving picture (an old interest not directly relevant to the project's development); 3) a filming of

Mongoloid children in a group (relation to project unclear); 4) analysis of humor and a ventriloquist and puppet (relevant to project) and; 5) the speech of a schizophrenic when a group member interviewed him.

This particular patient demonstrated so much confusion in the way he "framed" his messages and classified himself, it was uncertain even who he was or where he was from; the authorities doubted his statement that he was born "on Mars", and that his name was Margaret Stalin. Another patient interviewed also demonstrated a difficulty with classification. He would rarely speak, and when he did he would repetitively say, "I think my thinking is not good." (Haley, 1976a, p. 62)

At this phase in the project, the members had begun the development of a common language, the kernel of an idea, and appear to have been casting about for a realm of application. The interview of a schizophrenic patient early in 1953 appears fortuitous, a combination of being located in a psychiatric hospital and having a resident half-time psychiatrist on the roster. (The inference here is that it was Fry who conducted that interview.)

In 1954, Haley presented the project's first papers and a film by Bateson on otters, illustrating paradoxes in a variety of areas⁶, to the American Psychiatric Association in Mexico City (Haley, 1976a, p. 62). In general, during 1953, the group had shifted to emphasizing the potential conflicts between a message and its qualifier, so that in some instances, a paradox occurred.

⁶ Later published as: "Paradoxes in play, fantasy and psychotherapy," 1955, by Haley, and "A theory of play and fantasy," 1955, by Bateson.

Very important to this clarification of paradox was Milton Erickson's work with hypnosis. Erickson's work was apparently introduced to the project by Weakland and Bateson; Haley and Weakland began their series of what came to be annual meetings with Erickson in 1953. Erickson's re-conceptualization of hypnotic trance from an inner state or intra-psychic process, to a form of relationship with interpersonal processes, clearly constituted an important influence on the project, particularly with respect to the ideas of control and induction of "altered states." Haley's Uncommon Therapy: The Psychiatric Techniques of Milton H. Erickson, M.D. (1973) directly credits Erickson's contributions (as the various group members did at other times as well). Erickson's work with hypnosis is also evident in Haley's recurrent interest in the notions of power and control (to be discussed later in this chapter). Erickson's work with hypnosis was a crucial influence on the DB DM, particularly with respect to induction as an interpersonal process, which the DB later used with regard to explaining pathology, and for therapeutic strategies.

During 1953, then, the project shifted its attention from a multiplicity of areas relating to levels of communication, to the communication of schizophrenics. In the later months of 1953, they began to record schizophrenic communication (Haley, 1972, pp. 113-114; Weakland, 1969, p. 172). In 1954, the project shifted to a temporary preoccupation with schizophrenia.

1954 and the shift to schizophrenia. In 1954, the Rockefeller grant terminated and was not renewed (Haley, 1976a, p. 64), (although it is unclear whether they had applied for such a renewal). Haley indicates that partly for "practical reasons" (difficulty funding esoteric communicational research) and partly from "a developing tendency in that direction, it was decided to apply for a grant to investigate schizophrenic communication." (1976a, pp. 64-65). They obtained a two-year grant from the Macy Foundation.

During January of 1954, Bateson invited Don Jackson to join the project and the latter, though nominally a consultant, became a central member.

From the development of the project, it is clear that project interest in schizophrenia "was at first only an outcome of Bateson's prior interest in the general nature of communication" (Weakland, 1969, p. 171); that is, it was adopted in the course of normal science endeavors in other directions. Weakland (1969, p. 171) credits Haley with the shift of interest and the insight that the disturbed communication of schizophrenic patients was of potential interest to the group. During 1953, the group taped interviews with schizophrenic patients and paid close attention, not to the content of speech, but to the formal aspects of communication, which led them to perceive certain confusions in "discriminating the logical types of messages as characteristic of the schizophrenics we studied," (Weakland, 1969, p. 172); this research was regarded as anthropological in method, almost like a form of naturalistic

observation, rather than a clinical enterprise. (Weakland, 1969, pp. 171 and 172).

For the next two years (1954-1956), the project focused on the formal characteristics of schizophrenic speech. Haley (1976a, p. 65) presents a section of the Macy grant application.

The proposed research will focus upon an entirely different aspect of schizophrenic communication (from an approach in terms of symbols, delusional content, condensation, displacement, projection, etc.). It is generally agreed that schizophrenics have difficulty in discriminating between 'reality' and fantasy; and also that they have difficulty in the use of nonverbal and implicit signals. They may be oversensitive to such signals, they may ignore them; they may misinterpret them. The research, starting from these two generalizations, will study the schizophrenic's use of that particular category of nonverbal and implicit signals which indicate whether a given utterance is literal or metaphoric, jocular or serious, sincere or histrionic, etc. This class of signals we will call 'reality qualifiers.' (Haley, 1976a, p. 65)

Haley parenthesizes that during that period, "reality qualifiers" were also called "mode identifying signals;" they described the metamessage that describes, or frames, what sort of message the communication is.⁷ During this two year period (1954-1956), when formal aspects of communication were the explicit focus of their work, the project members had moved into a revolutionary position with respect to scientific activity. The focus shifted to schizophrenia and attention was paid to psycho-

⁷ This obviously constitutes language and concept formation by the DM, and both terms qualify as Kuhnian symbolic generalization.

analysis, if only obliquely (e.g., "symbols...displacement, projection, etc."). The group had begun the shift from what Griffith and Mullins (1972) call an elite to a revolutionary research group.⁸

A series of important developments ensued which eventually coalesced into the 1956 paradigm. After considerable debate the project members differentiated between "contradiction" and "incongruence." The term "contradiction" was used for messages which qualified each other in a conflicting manner at the same level, whereas "incongruence" was reserved to described messages that qualified each other in a conflicting manner at different levels. The emphasis rested on this latter case, as it was an example of conflict in the discontinuity of Russellian class and members; it was at the site of this class-member conflict that paradox was generated (Haley, 1976a, p. 65). The complexity of communication was becoming very apparent and attention began to be paid to the perception, or interpretation, of this complexity. Haley (1976a, p. 66) points out a complementary set of foci: ways of communicating and the interpretation of such communication. He states that when Bateson, in a letter to

⁸ An elite group is "recognized as being of central importance to their respective disciplines even while [it was] developing." (1972, p. 960) Obviously, while the project was a general communications research group, particularly as it was directed by Bateson who had just published a well-known book of research into communication, the DB project was an elite research group; with the shift in content area (schizophrenia) and focus (the formal aspects of schizophrenic communication), they entered a transition from an accepted elite group to a potentially revolutionary one. It is always a question at what point something becomes revolutionary: with the change in perspective, or with the publication? Griffith and Mullins' (1972) work proved helpful in this analysis and will be partially reviewed in Chapter VIII when the strengths and weaknesses of the Kuhnian applications are delineated.

Norbert Weiner in 1954, brought these two foci together in the context of levels of communication, the DB was first formulated.

First formulation. In writing to Weiner, Bateson first discussed language and metalanguage (qualifiers), paradoxes, and deutero learning (learning sets or learning-to-date), then formulated the prototypic DB paradigm, incidentally casting the injunctions into negative rather than positive form.

A question which I cannot clearly resolve is whether these two sets of two-typed communication [language and metalanguage; learning to learn] are really identical or are both independently operating in such phenomena as play.

As I understand it, type confusion leads to paradox when both message and metamessage contain negatives. On this principle we can imagine the generation of paradox in the deutero-learning system when an organism experiences punishment following some failure and learns that it must not learn that punishment follows failure. This would be approximately the picture of a man who having been punished for failure later is punished for showing his expectation of punishment after failure, e.g., is punished for cringing. (quoted in Haley, 1976a, p. 66)

The deductive quality in Bateson's thinking is quite clear here. As Haley (1976a, p. 67) has put it: "At this time, there was no example of a double bind drawn from natural-history data; it was a hypothesis about what sort of thing "must have happened in the life history of the schizophrenic given his confusion of Logical Types." The deductive quality of the formulation was repeatedly alluded to (Haley, 1959b, p. 358; Bateson, et al., 1963, p. 154).⁹ At least at this point, in a

⁹It scarcely need be mentioned that this completes the supplementary criteria for the development and characteristics of a Kuhnian paradigm.

1954 grant application, the paradigm was presented as being both inductive and deductive; "The theory is derived in part deductively from what has already been said about the capabilities [of schizophrenics], and in part inductively from experiences with schizophrenics and the literature describing their communication." (cited in Haley, 1976a, p. 66)

By the end of 1954, with Jackson now in residence for several months, the language becomes clearly more clinical. For instance, from that same grant application:

1. Aetiology. It is suggested that the base for later psychosis may be laid in infancy by the experience of dealing with a mother who both punishes the child for certain actions and punishes the child for learning that punishment will follow those certain actions, i.e., she generates paradox in the child by combining negative learning with negative deuterio-learning. (cited in Haley, 1976a, p. 67)

During this period, the project gradually abandoned the term "paradox" for "double bind."¹⁰ During 1955, the DB was defined as both a conflict between levels of messages and a conflict between levels of learning. The abstract logical puzzle became increasingly tied to behavior and the project continued to interview and tape schizophrenic speech. The unenviable position of the receiver of the double bind messages was considered and the 1956 "victim" was generated. Finally,

¹⁰ The term "double-bind" was coined by Gregory Bateson (Haley, 1972, pp. 113-114)

the article was written and published.

Development of the Relational Aspect of the Paradigm

Don Jackson joined the DB project early in 1954, at Bateson's invitation after the latter had heard Jackson give that year's Freida Fromm-Reichmann lecture at the Palo Alto V.A.¹¹ Jackson had spoken on his concept of family homeostasis, and Bateson, a member of the audience, approached him after the talk to say that the concept related to work he shared with Haley, Weakland, and Fry.

Jackson joined the DB group after an important controversy between Haley-Weakland, and Bateson over the nature of the paradoxical relationship. Bateson at first apparently argued that the relevant paradoxical relationship was specifically the mother-child relationship wherein the child was punished for expecting punishment; Haley-Weakland argued that this was the specific case of a general form of relationship relating to double negative injunctions and that the paradoxical relationship was pathogenic regardless of who the family member or significant other might be. Bateson, usually the most abstract thinker of the group, appears to have agreed with this change in content. Haley mentions

¹¹ Jackson's talk was in January of 1954 (Jackson, 1968, p. V). Prior to joining the DB project Jackson, Jack and Jeanne Block and Virginia Patterson had studied the families of neurotic and autistic children at the Langley Porter Neuropsychiatric Institute from 1953, Jackson ending his commitments at Langley Porter in 1956. (Jackson, 1965, p. 2 footnote)

that he, Bateson and Weakland "spent two days in seclusion in the mountains outlining the research to be done." (1976a, p. 67) Obviously, Jackson was not there. He is, however, credited in the 1956 article, which was developed and written in 1954 and 1955, and which depends in large part on his ideas regarding family homeostasis. In terms of a Kuhnian analysis, the article could not have been formulated without the homeostatic concept he provided. At times, the necessity of both the communicational and the relational concepts was appreciated by Jackson and Weakland (1961, pp. 15 and 16), for instance.

Jackson's 1954 formulation. Jackson presented the Freida Fromm-Reichmann lecture in January of 1954, at which time he spoke of his concept of family homeostasis. He then presented the concept at the American Psychiatric Association meeting in St. Louis, Missouri on May 7 of that same year (Jackson, 1957b, p. 79); the formulation finally appeared in written form in 1957.

In this article (formulated in 1954), Jackson alluded to the work of Horney, Sullivan and Fromm in elucidating the importance of interpersonal processes and addressed himself to interactional patterns within the family which serve to maintain family constancy. His concept of homeostasis was explicitly based upon Claude Bernard and Cannon's work on homeostatic mechanisms which maintain relative constancy in the body through the interplay of dynamic processes (Jackson, 1957b, p. 79). Jackson regarded "family homeostasis" as "depicting family

interaction as a closed information system in which variations in output or behavior are feedback in order to correct the system's response." (pp. 79-80) Such homeostatic mechanisms operate implicitly and re-establish the status quo. Examples of such mechanisms in a family might occur "whenever the wife shows a certain degree of resentment, her husband deprecates himself; whenever...parents quarrel, their child diverts them by becoming troublesome; whenever another child shows a certain degree of independence, his mother labels it as dangerous or disturbed." (First, 1975, p. 9)

Jackson's purpose in the paper was to address to particular types of family interaction patterns: 1) those changes that develop in other family members as a result of changes in the identified patient during psychiatric treatment and 2) the relation of specific family interaction patterns to psychiatric nosological categories (1957b, p. 79) The first category, obviously addressed one of the central anomalies in the treatment of schizophrenia, the emergence of pathology or disturbance in a hitherto healthy family member coincident with the improvement of the identified patient in treatment. A variant of this is also considered: "Most of us are acquainted with situations where one person has started treatment, and soon the entire family has been parcelled out among the circumambient psychiatric brotherhood." (1957b, p. 84) (The wording here, as elsewhere with Jackson, is quite Sullivanian.)

Pursuant to the question of etiology, Jackson contended that

schizophrenia originated, not in constitutional factors, and not in the Freudian idea of the single traumatic event, but rather in the concept of repetitive trauma. Such repetitive trauma was the result of distorted interpersonal processes not isolated events, and were a contemporaneous rather than historical question "not [of] who does what to whom, but how who does what." (1957a, p. 184) Bateson, et al., develop this idea in the 1956 paper deductively, wondering under what conditions or continual processes a person must live, to develop schizophrenia.

Jackson addressed other anomalies as well: the necessity of seeing a child's "significant-others," (1957b, p. 81) and neurotic complementarities. (Jackson's manner of stating the latter was rather different: "The fact is sometimes overlooked that one reason many of us continue to manifest our neurotic woes is that we manage to find people with whom to integrate on a neurotic level." (p. 813; his emphasis) These points, especially with respect to the reciprocity of health in schizophrenic families, of course meets the Kuhnian criterion that a paradigm address the anomalies of the previous system (as the communicational aspect addressed the mystery of schizophrenic "word salad", investigating the form rather than the content of communication and homeostasis).

The homeostatic mechanisms invariably involved interpersonal processes and at least two, and usually three, people. Jackson's illustrations were always more clear if several people were involved (and in

fact, some family therapists prefer working with a large rather than small number of people as it makes it easier to both spot and intervene in typical family patterns). This is well illustrated in one of Jackson's examples (1957b, p. 82).

The paternal uncle of a woman patient had lived with her parents until she was ten when he married. Her mother's hatred of him was partially overt; however, his presence seemed to deflect some of the mother's hostility toward her husband away from the husband, and the brother gave moral support to the father. Following this uncle's departure, four events occurred that seemed hardly coincidental: The parents began openly quarreling, the mother made a potentially serious suicide attempt, the father took a traveling job, and the patient quietly broke out in a rash of phobias.

The role of the third, or fourth, person in a system, and not only the relationship of patient and symbiotic parent, was repeatedly emphasized by Jackson; until his work, the dyad had been regarded as a closed system; with the introduction of Jackson's concept of homeostasis, the system opened up to include the entire family and the theory was cast in systemic terms.

Subsequent development of the homeostasis idea. The ideas first forwarded by Jackson in 1954 were developed somewhat in the 1956 article, then in a number of other articles between 1956 and 1968; though he wrote many other papers, only a portion were on the DB per se (e.g., 1960, The Etiology of Schizophrenia), rather than a development of the relational aspect of the paradigm, while still others were elaborations of earlier interests (e.g., 1958, "Guilt and the control of pleasure

in schizoid personalities"). Several papers, however, do stand out as developments of the homeostatic and/or relational aspects he contributed to the DB paradigm.

In 1957(a), Jackson extended his concept of repetitive trauma, rather than the isolated traumatic event, as responsible for schizophrenia.¹² Two following papers are in some sense collaborations with Weakland, who seems to have fully appreciated the homeostatic implications. In 1958, they collaborated on the idea of the repetitive trauma, by documenting the precipitation of a psychotic episode by double binding communication which occurred in the father-mother-adult-"child" triad. Weakland wrote a paper on the DB and three-party interaction (obviously homeostatic) in The Etiology of Schizophrenia (1960) which Jackson edited. The extension in that paper was from the dyad to triad, an elaboration of Jackson's 1954 work on the third person in relation to the usual pathogenic dyad. Later, in 1965, Jackson published "The study of the family," which was a further elaboration of the homeostatic theme.

Soon thereafter, (1965), Jackson and Janet Beavin wrote and published "Family Rules: Marital Quid Pro Quo" in which they examined the

¹²The dates of publication might engender some mild confusion about this point. It might be asked how a 1957a paper could elaborate upon a 1957b? The problem dissolves when it is recalled that the 1957b paper was published in 1957, but written and at least twice presented to public forum in 1954 (once in January at the annual Freida Fromm-Reichmann lectures and later in May at the American Psychiatric Association's annual meetings). The article on family homeostasis was published in part for historical reasons, in 1957 and only by accident later than its progeny.

development of family rules, both implicit and explicit and some of the typical pathologies of rule conflict (Watzlawick and Weakland, 1977, p. 20). This latter point was obviously an extension of his 1954 work in which his second focus was the correlation between typical interpersonal patterns and nosological categories. Their primary point with respect to rules was that some rules were the result of something like a bargain between the couple (hence the "quid pro quo"); yet other implicit rules emerged from the partner's interactions and not from any individual decisions or actions, so that the partners were rarely aware of them and neither could be perceived as having any real intentionality in their establishment (Watzlawick and Weakland, 1977, p. 20).

What is regarded as Jackson's final comprehensive presentation (Watzlawick and Weakland, 1977, p. 193), "Schizophrenia: the Nosological Nexus," was initially presented to the First Rochester International Conference on "The Origins of Schizophrenia" in March of 1967.¹³ In this, Jackson developed several points demonstrating that individual personality theories and treatment were insufficient for schizophrenia and also demonstrated the correlation between the identified patient's behavior and typical family interaction patterns; this latter point is an extension of both his work with correlation between family patterns and nosological categories (e.g., 1954 and 1965b), and also of Jackson's and Weakland's 1958 paper on the precipitation of an acute schizophrenic

¹³"Dr. Jackson died suddenly on January 28, 1968." (Publisher's note, in Jackson, 1968, p. viii)

episode.

Finally, he introduced the concept of "restrictiveness" as a major factor in family systems processes. "Restrictiveness" applies to an implicit family rule against changing family roles, which are in themselves tacit. This in itself creates a paradoxical situation (Watzlawick and Weakland, 1977, p. 193). The concept of restrictiveness is operationalized by homeostatic mechanisms, and constitutes one of characteristics of particularly powerful sets of homeostatic mechanisms. This point both extends the homeostatic notion, and addresses a characteristic of schizophrenic families recognized by several researchers-- a marked tendency toward difficulty in changing an almost obdurately dynamic system in which, the more things changed, the more things stayed the same. Wynne, et. al., (1958) refer to a "rubber fence" by which such families seem to allow the therapist to enter, but not in fact allow him/her any impact. Bateson (1961, p. 119) also addressed this point with regard to family development over time.

The families containing schizophrenics exhibit a stability that is, in general, not present in normal families. Many descriptive statements about the relationships between members remain true much longer than in usual families. Indeed, these statements may be said to be stable under the impact of the processes of maturation of the independent members. The growing up of the identified patient and the senescence of the parents scarcely seem to affect the patterns of behavior between parent and offspring. Overprotectiveness, if present, continues undiminished and the incessant inconsistencies of relationship that we have called "double binds" continue unabated.

Obviously, the homeostatic mechanisms that comprise restrictive-

ness account for one of the classic anomalies with schizophrenia, i.e., the low rates of permanent cure or substantial amelioration and the very high rate of relapses.

In short, Jackson's primary lines of development with regard to homeostasis, beginning in 1954, addressed three-party interactional processes (with some elaboration by Weakland here) and systems dynamics, family rules and cybernetic patterns of feedback and correction, the correlation between repetitive interpersonal patterns and certain nosological categories and the rule of homeostasis and restrictiveness in relapses and poor therapeutic results with schizophrenics. By his 1967 paper, Jackson was explicitly referring to cybernetics, particularly with respect to homeostasis; he appeared comfortable and facile with General System Theory, referencing von Bertalanffy and using several of his systems concepts, (e.g., "equifinality") as well as Garfinkel (1964) and the ethnomethodologists' (also in California) concepts (e.g., "constitutive rules"), and Sullivan with respect to interpersonal patterns and clinical nosologies. He was clearly attempting to provide, or to tie-in to, a meta-theoretical framework; the DB project had dissolved and many of the differences in scientific and/or clinical approach he had had with other members had widened and become elaborated. By the time of his death, Jackson had widened the conceptual and clinical breach between himself and the mainstream DB position to such an extent, he was developing an intellectual approach traditionally at odds with that used by the DB. As review of Jackson's precursors, and

development will make clear, that separation was not at all coincidental.

Precursors and influences on Jackson's work. Jackson's background and influences were quite different from the other three paradigm-makers' intellectual antecedents. Jackson, first of all, was an experienced clinician. Neither Bateson nor Weakland had had any such experience and Haley had begun some clinical work around 1952 or 1954 (though it is unclear where or when he received his training or what type of clinical work Haley was doing). Jackson's experience as a clinician, of course, provided the basis and the empirical material for his homeostasis concept. It also apparently impelled him to deal with the clinical community years before the other three paradigm authors. It was Jackson who, as early as 1957(a), addressed the issue of repetitive traumatic conditions vs. the classical psychoanalytic single traumatic event, for instance. Similarly, analogies and differences between DB therapy and other traditions, especially psychoanalytic, continued to interest him. (See, for instance, Jackson, 1961a, pp. 270-278).

His clinical experience also contributed greatly to the success of DB therapy; Jackson provided a perspective that the three others probably could not have brought to treatment. For instance,

Manipulations or interventions must not be employed if the therapist has any negative feelings toward the patient and if they are especially counter-indicated as a way out of some sort of therapeutic impasse. That is, when the therapist feels that he does not know what is going on, things bog down and then a bright idea occurs to him. This is exactly

the time when an intervention should not be used. The therapist must frankly answer any questions that the patient asks in an attempt to understand the meaning of his recommendations. The therapist must be prepared to be wrong if his intervention fails or if the patient becomes angry...He should accept his position, non-defensively and be free to lose a patient.

I hope these remarks will not be interpreted as a "cook-book course," designed to get around the need for intensive training in order to do psychotherapy; the opposite is the case, for only an experienced therapist can tailor an appropriate intervention to each individual patient.

This is one of the paradoxes, and one of the stumbling blocks in psychotherapy: the experienced therapist is best able to conceive and execute innovations; yet his experience is apt to render him chairbound and a devotee of his own style. (Jackson, 1961b, p. 259; his emphasis)

Jackson had, according to his own account (1957a, p. 181) become interested in the etiology and pathogenesis of schizophrenia since 1943 when Jacob Kasanin first introduced him to this area. During the ensuing years, Jackson saw schizophrenic patients in a variety of therapeutic modalities ("...I saw schizophrenic patients in collaborative therapy, group therapy, intensive individual therapy, and multiple therapy.", 1957a, p. 181), yet felt he'd not any real understanding of the origins of the disorder until the DB project.

His own concept of family homeostasis was sparked by an early paper on the treatment of schizophrenia by R. Laforgue (1938).

Laforgue mentioned that at a significant point in his female patient's therapy her sister (with whom she lived) became severely depressed. He attributed the sister's difficulty to a manifestation of the same unfortunate genetic structure that had caused his patient's schizophrenia. He did note that the sister's depression was coincident with a sudden improvement in his patient. (1957b, p. 88)

In point of fact, Laforgue's article did somewhat more than that. Although he did identify the processes as homeostatic, Laforgue clearly described two homeostatic situations--one in which the "healthy" family member declines with improvement of the identified patient, thus revealing latent depression (usually), and also a situation wherein the individual patient's disorder serves as a screen behind which another family member can avoid a loaded developmental or interpersonal task. In this admirable excerpt, Laforgue also draws attention to substitution for lost objects which Normal Paul and also Peter Mueller regard as important in the etiology of schizophrenia.¹⁴

¹⁴In short, Odile's [identified patient] behavior when faced with any sort of danger was different from what it had formerly been. In the old days danger had had an irresistible attraction for her, but now she avoided it: she had become as timid and fearful as a child.

This change was, from my point of view, highly satisfactory but for one uncomfortable obstacle. For some time I had been reflecting on the reactions which Odile's progress might possibly produce in the elder sister who was still looking after her. The more the patient improved, the gloomier did her sister become and, after the latest transformation in Odile, the gloom deepened into definite depression. This reaction might seem paradoxical to anyone who has not studied family-neuroses very closely. But it did not, in fact, take me by surprise. I had noticed at the beginning of the treatment that the elder sister who cared for Odile so devotedly was terribly jealous of her authority over the patient. This devotion, manifesting itself in a spirit of complete self-sacrifice on behalf of her sick sister and in a strong inclination to give up having any life of her own, had struck me as suspicious. When examined more closely, these symptoms revealed a powerful attachment to her sister, an attachment which she had in the first instance displayed towards her mother and which, when the latter was taken from her, had prevented this daughter from developing in the direction of a normal family-life. Like the patient, the elder sister, though quite capable of an occasional flirtation, had eliminated men from her affective

The mother implied that this was a friend of Barbara's although it had been the mother, in fact, who had invited this girl to stay with them. Barbara exhibited some paranoid symptoms, speaking about spies, and the mother wrung her hands and said, "She's getting worse." In that session, I asked Barbara, "Are you talking about Genevieve acting as a spy for your husband? That she is going to tell him what is going on in the house and whether she approves of the way you treat your child, etc.?" To this, Barbara was able to reply quietly, "Yes." The parents keep insisting that this was a friend of hers, that it was a silly idea, but I suggested that since the divorce was not final, and since the husband might ask for the custody of the child, perhaps it was not silly to look on Genevieve with suspicion. I suggested that they have her stay in a nearby motel, rather than in an already crowded house. They also had no idea of how long she would stay because they failed to clarify this. (Jackson, 1961a, p. 284)

Essentially, the double binding and homeostatic maneuvering served to reinforce the idea of the patient's lack of decision-making ability and the idea of her as impaired or disordered, while maintaining her in a non-adult position, thus making it well nigh impossible for her to leave or disengage.

The homeostatic concept also addresses two important clinical issues regarding the etiology of schizophrenia. It helps to answer the questions about what researchers concerned with longitudinal risk for schizophrenia refer to as "high-risk invulnerables," that is, those individuals at risk for schizophrenia along some number of dimensions, who do not develop the disorder. Jackson (1961a, pp. 281-282) facetiously points out when the sibling of schizophrenics have been investigated, they are usually preceived as "fine" and the conclusion has been that schizophrenia is a recessive disorder, since it only strikes one in four or

Obviously, Laforgue was perceptive and therapeutically acute; Jackson developed the idea of reciprocity of health into the formal concept of family homeostasis. The route of that development, however, is relatively obscure. Two points of importance in the development stand out: Sullivan's influence and the shift to a family interest.

Jackson had studied, after his psychiatric residency, at Chestnut Lodge, a private psychiatric hospital in Maryland, equally well-known for its treatment of schizophrenics, and its clinicians, especially Harry Stack Sullivan, Freida Fromm-Reichmann and Otto Allen Will. Sullivan, as already mentioned, was the originator of interpersonal (or relational) psychiatry. It is obvious that Jackson was pervasively influenced by Sullivan's work, although it is unclear whether that occurred before, or during Jackson's stay at Chestnut Lodge. At several points, Jackson acknowledged Sullivan's influence explicitly (1961b, p. 268). The concept of homeostasis, for instance, is a relational or interpersonal one, both dynamically and in its origins. The content of Jackson's work also expresses some of the same concerns or assumptions: attention paid to a person's "security operations"; the correlation between certain interpersonal processes and nosological categories (see especially, Sullivan's Clinical Studies in Psychiatry, 1956 and Jackson, 1957b); the

experience; she had used Odile as a screen, sheltering behind her and thus giving her own life a facitious purpose.

I made use of this attachment as long as it was helpful in Odile's treatment, but now it was becoming a hindrance. Psycho-analytical experience constantly teaches us that neurotic parents can check the affective development of their children. Often, to cure the neurosis of the latter we must treat the former. (Laforgue, 1938, p. 158)

importance of the concept of low self-esteem in schizophrenia and concomitantly, the importance of having and showing respect for the patient (Jackson, 1957a, p. 184).¹⁵ At times, in fact, Jackson's wording in papers is vividly reminiscent of Sullivan's rather wry, and at times notoriously difficult style. (For instance, when Jackson refers to the "circumambient psychiatric brotherhood," or when he describes the expression of hostility towards one's mother as an activity "which among sophisticated people is a rather common indoor sport," (1961b, p. 258.) Examination of Jackson's work before the DB project (e.g., "Some factors influencing the Oedipus complex," 1954), during (e.g., "Guilt and the control of pleasure in schizoid personalities," 1958), and after (1963-1969) clearly reveals Sullivan's influence and Jackson's retention and elaboration of the interpersonal approach in systems terms. It was this factor, more than anything else, that resulted in the split in the DB DM in 1959.

Jackson's shift from primarily individual clinical work to family work followed his change in working context from Chestnut Lodge to Palo Alto.

I became interested in family therapy about seven and a half years ago, when I went from Chestnut Lodge to Palo Alto. At Chestnut Lodge, we had treated schizophrenics

¹⁵My thanks to Dr. Harold Jarmon for pointing out that Sullivan frequently alluded to the dynamic equilibrium of relationships, the "quid pro quo" aspect of relationships and the crucial role of communicational processes in relationships.

with psychotherapy, so I of course did so in Palo Alto. But, at the Lodge, we had never had anything to do with their families. This, I understand, has changed somewhat now. But in my day, it was really a disgrace for the therapist to encounter the parents. This was something he avoided and always left to the administrator. In Palo Alto, which is a small university town, I couldn't avoid the relatives; and this led to a lot of surprising and sometimes not very pleasant results. I became interested in the question of family homeostasis, which seemed most marked in the families where a schizophrenic patient was able to live at home. If he then went through psychotherapy and benefited from it, any move on his part would usually produce all sorts of disruptions at home. Surprisingly, there is very little written on this topic. It is something that people who do psychotherapy can confirm, and to which they can add horror stories of their own of what occurred when they undertook psychotherapy with schizophrenics and ignored the patients' relatives. (Jackson, 1961a, p. 272)

It was in early 1954 that Bateson approached Jackson after the latter's Fromm-Reichmann lecture in Palo Alto, and invited his participation in the just developing DB project.

Importance of the homeostatic aspect. The importance of the homeostatic aspect occurs in two different areas: the first in Kuhnian terms and the second in clinical terms. First, Jackson's concept of homeostasis addressed several anomalies including: reciprocities in mental health, (particularly wherein the identified patient improved and a family member became disordered); the exceedingly high relapse rate in schizophrenia, and the general lack of therapeutic success with schizophrenic patients.

Homeostasis served three functions with respect to a Kuhnian paradigm, which the communicational aspect alone could not. First, homeo-

stasis explained why, in two families with binding communication, one might have a schizophrenic offspring and the other might not; if a family, even if double binding, had a reasonably healthy parent, or even parent surrogate who could intercede, a child was decidedly less likely to become schizophrenic than in those families where the homeostatic mechanisms precluded rather than fostered that benign third-party contact. Such third-party contact could ameliorate the pathogenicity of the situation despite the fact that it did not eliminate the double binding.

Secondly, the homeostasis provided the foundation and operation by which double binds could be made applicable to human affairs in a non-trivial way. Bertrand Russell had been concerned that, while the Theory of Logical Types was interesting, it might have been irrelevant for anything significant (Abeles, 1976, p. 118). Bateson apparently suffered similar qualms insofar as he, Weakland and Haley felt the first important question facing the early project (after they had developed a common approach), was whether the "paradoxes of abstraction were relevant to anything important in human life?" (Haley, 1976a, p. 60) Homeostasis provided the non-trivializing element by providing the DB with an interpersonal, or relational arena. A common source of bewilderment with the DB hypothesis has centered around the question: "Why don't these people just leave?" That is, if double bind communication is so pathogenic and aversive, why don't the "victims" merely leave the scene?

Such questioning reveals a lack of understanding with respect to the homeostatic element. Abeles (1976, pp. 115-116) appears to understand this element quite well. She emphasizes:

Double bind theory is about relationships, and what happens when important basic relationships are chronically subjected to invalidation through paradoxical interaction. Such things are difficult to illustrate; double binds are so imbedded in the relationships of which they are a part that they are hard to see. In any relationship with a history, the factors are so many and so varied that an observer is likely not to know what many essential referents of a given statement may be, even if explicit verbalizations were the main means of communication. Though every communication says something about the relationship of the communicants, examples which can illustrate the invalidation of a relationship with a few exchanges of remarks and gestures are difficult to find. One speaks of "binds" in referring to isolated incidents which exemplify the pattern. However, the concept can be better appreciated in its proper, more encompassing application: the relationship is a bind. Isolated examples of double binding communication illustrate the relationship, characterized as it is by chronic threat of invalidation. (her emphasis)

If it is further recalled that any appropriate within-paradoxical frame response is necessarily paradoxical in turn, then an implication is that over the course of years, a person has learned that structure of relationship (the reverberating cycles, or spirals of binding). To leave a DB, essentially means to leave the relationship. The person in a double bind remains in a bind "to preserve an essential relationship." (Abeles, 1976, p. 120) The relationship is essential because by nature of the DB, neither party in a dyad can gain closure, nor have the relational needs been met, particularly if one of the participants had been double bound as a child. As Abeles points out (1976, p. 121), this

has often been misconstrued as "dependence." More abstractly, but just as inevitably, if an individual were to grow up in a family being double bound, s/he would learn both that this was the structure of relationships, and would learn to double bind in turn, as a participant in the spiral. If such a person could gain enough closure, self-esteem, maturation (whatever) to begin a relationship and family, naturally both his/her expectations and communicational pattern would lead to another double binding family.

The third function served for the paradigm includes areas of sensitivity in each family. Those areas around which homeostatic mechanisms come into play, and with which double bind communication is used, point to particularly sensitive family areas. (This is potentially an area of link-up with another family framework through the concept of the "family secret.") Jackson provides an illustration of both such mechanisms being brought into play around the area of the patient's ability to make decisions.

One of the most rewarding occurrences in family therapy is the concordance between a symptom in the patient and a piece of family interaction that explains the symptom. In this sense, I am stating that schizophrenia or schizophrenic symptoms are adaptive behavior. For example, Barbara's parents complained more about her indecisiveness than about anything else. Evidently, it is a problem when she gets up in the morning as to what she will wear and everything else in the day becomes a similar crisis. When one listens to the sessions, it is striking that when Barbara makes a decision, the parents refute it in some fashion and then she backs out. Yet, they in no way see themselves as having anything to do with her indecision. One striking example was an incident in which a friend of her ex-husband was going to visit them.

so siblings. His homeostatic view would question both the well-being of the siblings and the etiological formulation. For those family members who remain well even if the patient improves, homeostasis would predict the presence of a relationship with a third-party which attenuated the effects of the special relationship to the symbiotic parent.

Similarly, this explanation accounts (at some level) for why one child in a family is chosen or overlooked for the symbiotic relationship, or the ameliorating third-party bond. At a more fundamental level, however, the homeostatic process cannot, it seems, account for parental choice in symbiotic object.¹⁶

DM Development from Paradigm to Dissolution

Between 1956 (paradigmatic statement) and 1962 (dissolution), the DB DM was extremely productive; from 1959-1962, it was divided into two institutions, yet maintained its productivity. It was during the 1952-1959 period that the paradigm was elaborated and the major portion of DB work done, yet the differences in intellectual traditions eventually resulted in a schism between Bateson's Communication in Schizophrenia¹⁷ project and a project formed by Jackson in 1959, the Mental Research

¹⁶ It does seem to me, however, that I. Boszormenyi-Nagy's (1970) framework can account for such a choice.

¹⁷ This was the official name by which the project was known.

Institute. In retrospect at least, two powerful trends in early and middle DM development emerge: there developed an increasing emphasis on describing family organization then changing it (i.e., doing therapy) and with this shift came a widening split between the adherents to the communicational and the relational aspects of the paradigm.

Focus of family organization. During 1953, the project had begun to tape "schizophrenic communication as part of their early communications research (Haley, 1972, p. 114). In 1954, project members visited John Rosen, (a therapist well-known for his approach with schizophrenics) and began to study a number of psychotherapists (Haley, 1972, p. 114). During 1955, a good deal of time was spent in formulating and refining the paradigmatic 1956 statement, but in February, 1956, the project brought in the parents of a patient who consistently panicked within several minutes of each parental visit to the hospital (Haley, 1976a, p. 72). This first in vivo family session occurred in 1956. Up to this point, research on schizophrenia had been largely deductive, with some induction work on the audio-tapes. With the inclusion of this particular parental pair, the DM embarked on their future mode of focusing on the actual behavior of families. At this point, they began using video-tapes, in response to the need to adequately capture the multiple levels of communication, particularly the non-verbal aspects (as they often functioned as qualifiers). Their first documented full-family interview occurred sometime during 1957.

With the advent of in vivo family sessions, several problems began to emerge. Most obviously for this project, was the necessity for developing an adequate descriptive language for the processes they were now observing. In attempting to describe more fully the actual family processes they were filming, the project tried studies using kinesics and vocal intonations, but these apparently became dead-ended (Haley, 1976a, p. 73). The 1958 Weakland and Jackson article on the precipitation of a schizophrenic episode appears to be an expression of both the attempt at description and the newly worked out specific connection between double binding and acute psychotic reactions.

With the new focus on actual family behavior, differences among DM members which had been inherent but quiescent became more evident and divisive. Haley (1976a, p. 73) perceived the differences in terms of "...what aspect of communication to focus upon and what terminology and theoretical models to use." A split developed between a "strictly-communicational approach dealing with the description of observable messages in terms of Logical Types," and an "internal processes wing" which emphasized the codification of messages, or the internal processes of the receiver, and centered on perception and learning." (Haley, 1976a, p. 73). Though implicit, it is obvious from Haley's account that he belonged to the first group.

The first group, termed the "behavioral wing" by Haley, argued for attention only to observables, (in this case, "strictly observable messages") (p. 74) in the interests of avoiding "age old problems of

psychology related to the internal processes of individuals. Observation and verification were positively valued. "Description" applied to the interchange of messages.

If it is said that a schizophrenic is faced with certain messages misinterprets or misperceives them, the description must end there. If his response is also described in terms of messages, the descriptive system becomes more complete. (p. 74)

Such a series of descriptions could lead to a systems theory of human beings (which was the explicit goal of both wings, according to Haley [p. 74]0. They also preferred to not generalize the DB pattern, feeling it would lose explanatory power.

The "internal processes" wing argued that this appellation was a misnomer because the distinction between a message and the codification of a message by the receiver was fallacious. They later argued that the terminology should be established as a higher level of generalization than the description of messages (Haley, p. 73), in other words, that there be developed "meta" concepts. (For this reason, they were characterized later by Haley (p. 74) as the "higher generalization" wing). Thus, they objected to restricting the double bind specifically to the interpersonal patterns pathogenic for schizophrenia and preferred to use it more broadly, even to help explain evolutionary processes and the telencephalization of the brain (p. 74). Concepts used comfortably by this group apparently included: "learning, perception, awareness, expectation and the language of emotion" (P. 73). At issue were such concerns as whether the receiver was aware, or was perhaps misperceiving

or misinterpreting, and what his/her experience was. Clearly this wing included a phenomenological element. One infers Jackson helped constitute this wing, because of his occupation with interpersonal processes and clinical work; Bateson may also have had some participation here--the emphasis on higher generalization would appeal to him. Though it is more difficult to guess what his stance would be with regard to phenomenology, he differed sharply with Haley (Bateson, 1976, p. 106) over the power dynamic, so perhaps he was not averse to phenomenological concerns. Balanced against this is his predilection for abstracted systems approaches, and the integration of experience and systems might or might not have seemed dubious to him at that point. Obviously, Bateson's position here is unclear; so is Weakland's. It is difficult even to speculate about the latter's position here, though it would not be surprising if he were wholly identified with neither wing and in fact, served a mediating function which helped the project to produce as much as it did over the next six years. Certainly, some cohesiveness remained; it would not be surprising to find out that Weakland had supplied some of it.

What is surprising is that the rift continued. "The higher generalization wing argued that the behavioral wing was too narrow in approach and the behavioral wing argued that the other was too diffuse and ambiguous to have useful application to the data." (Haley, 1976a, pp. 74-75). These differences in approach began to appear in papers in 1958, the next year that the project members published (Haley, p. 75)

The rift next expressed itself in the project's conceptualization about family organization during 1956 and probably 1957 also. Again, the pressures of empirically observing families brought out fundamental characteristic differences among project members that their earlier, more abstract work had allowed to remain submerged. Haley (1976a, p. 89) points out that "within a general framework of agreement there was considerable agreement." Disagreement arose over choice of theoretical model, whether to focus on total family processes or partial family processes, and differences in assumptions regarding motivations of family members.

Characteristic individual tendencies. With regard to family organization, Haley (1976a, pp. 89-90) has pointed out that any description of a family must hang upon "some implicit or explicit analogy" (paradigm?) and with the shift from individual to family processes, the previous psychiatric analogies were inappropriate. Though there was project consensus that the preferred model was some form of homeostatic system that involved levels, there was debate about what specific form it should take. Bateson wanted to use some adaptation of Von Neumann's Theory of Games despite protests from other project members and his own awareness that the assumptions of Game Theory would not work for family interaction. Haley wanted a model using homeostatic organization where rules for the family were Level I and rules for who was to make the family rules at Level II; Bateson objected to the assumption that a family should be described in terms of members seeking to control other

members. He preferred that Level II should be regarded in terms of the calibration of habit in the individual, or habitual patterns in the family. Haley's position gradually became more emphasized than Bateson's in the DM, except in Bateson's own work.

With regard to what aspect of family organization should be focused upon, Bateson's preference was to emphasize the DB as a characteristic sequence in schizophrenic families, then to generalize that sequence to other phenomena whereas other project members preferred to focus on patterns of families' organization, almost a set of typologies. Though Haley does not elaborate on the origin of this latter position, it appears related to Jackson's interest in correlations between characteristic interactional patterns and psychiatric nosology. Here Bateson's position apparently prevailed.

Each project member had a different position on the issue of individual motivation, that is, on assumptions about why people in families did what they did. Haley preferred the idea that people did what they did in order to control, govern or influence events in relation to other people; Weakland proposed no motivation for parents posing a double bind to a child, but emphasized the motivation of attempting to conceal it once it was done; Bateson posited both his original idea that a central motivation was concealment, (with punishment if concealment was breached) and also ascribed to more traditional concepts for motivation, e.g., fear, hate, love, threats of punishment and avoidance of pain (Haley, 1976a, pp. 90-91). Jackson's position is not reviewed by Haley.

What is quite obvious, however, is that the positions taken by each member during these two major controversies was characteristic for the individual and relatively consistent with his previous and future work. For instance, Bateson's position favored abstracted formulations and a largely (though of course not completely) deductive approach; Haley has what seems to me an apt description of Bateson's approach.

Bateson's lifelong preference, in general, was to choose a characteristic sequence and to relate that sequence to a variety of situations in the field of science. [For example] his preference was to emphasize the double bind as a sequence which occurred in schizophrenic families and to generalize that sequence into the field of evaluation, biological processes and so on. (1976a, p. 90)

It would make sense that Haley in particular would notice Bateson's abstractedness. One of the prevailing values of the DB DM was a strong positive valuation on efficacy; the practical, effective and/or productive was highly regarded. Therapy was regarded as a form of problem-solving (e.g., see Haley's Problem-Solving Therapy, 1976b) and the project prided itself as having had success with families that had been therapeutic failures in other therapies. The positive valuation on efficacy was certainly held by all the DB members, but none with such a single-mindedness as Haley. Haley's style was to home in on a problem and obliterate it, often with what feels like paradoxical judo, always re-framing a situation into the guise of a problem circumscribed enough to solve. Haley was particularly good at getting change in obdurately irrational and paradoxical families.

One of the concomitants to this concern for efficacy, however, was Haley's lasting preoccupation with the power dynamic. His work during the formal life of the DM and afterwards centers about the issue of control. His early interest in hypnosis displays an interest in control, particularly of the paradoxical kind. In 1957(a), Jackson (footnote, p. 184) cites Haley's help for "the idea that the control of the definition of a relationship might be used as a descriptive tool for depicting family interaction."¹⁸ In 1959(a), Haley developed the control issue more fully, crediting Bateson with the communications hypothesis that it is difficult for a person to avoid defining, or taking control of the definition of, his/her relationship with another (1959a, p. 324). In other words, a person cannot not qualify a message (1959a, pp. 323-324).

A person must speak a verbal message in a particular tone of voice, and if he says nothing, that, too, is qualified by the posture he presents and the context in which his muteness appears...For example, if a person is silent when he is expected to speak, the silence becomes a qualifying message, and if a man neglects to kiss his wife good-bye when she expects it, this absence of this movement qualifies his other messages as much as, if not more than, the presence of it.

The only way, according to Haley's analysis, that a person can avoid indicating what is to take place in a relationship and therefore

¹⁸Note the 1956-1957 concern with descriptive language for family processes.

avoid defining it, is to incongruently negate some crucial part of his statement along any of the four formal characteristics of any message (1959a, p. 325):

- (1) I
- (2) am saying something
- (3) to you
- (4) in this situation

A person can incongruently negate any of these characteristics and avoid defining the relationship. However, a summary of these ploys reads like a list of schizophrenic symptoms:

The incongruities may be crude and obvious, like the remark, "My head was bashed in last night," made by a patient whose head is in good shape; or they may be subtle, like a slight smile or odd tone of voice. If the patient denies that he is speaking, either by referring to himself in the third person or calling himself by another name, the psychiatrist notes that he is suffering from a loss of identity. If the person indicates that "voices" are saying these things, he is described as hallucinating. If the patient denies that his message is a message, perhaps by busily spelling out his words, the psychiatrist considers this a manifestation of dissociated thinking. When the patient denies that his message is addressed to the other person, the psychiatrist considers him delusional. If the patient denies his presence in the hospital by saying that he is in a castle or a prison, the psychiatrist notes that he is withdrawn from reality. When the patient makes a statement in an incongruent tone of voice, he is manifesting inappropriate affect. If he responds to the psychiatrist's behavior with messages which qualify that behavior incongruently, he is autistic. (Haley, 1959a, p. 327)

These linguistic maneuvers are implicit ways to avoid defining a relationship or the behaviors to take place as constituents of it. In fact, Haley (p. 324) regarded interpersonal relationships as classifiable

with regard to the ways people used to deal with definition (i.e., control), problems.

The strategies and ploys devised by Haley in his therapy with schizophrenic families exemplify the issue of control to the point that the DB approach has been criticized in this area (First, 1975). Haley's more recent works, (e.g., Problem-Solving Therapy, 1976b) use the issue of control rather unabashedly, excusing it as merely explicit in DB therapy and implicit in other therapies. Bateson, again being consistent with his position throughout DM development, took serious exception to the concentration on control.

...Haley slides too lightly over very real epistemological differences between himself and me. As I saw it, he believed in the validity of the metaphor of "power" in human relations. I believed then---and today believe even more strongly---that the myth of power always corrupts because it proposes always a false (though conventional) epistemology. I believe that all such metaphors derived from pleroma and applied to creatures are antiheuristic. They are a groping in a wrong direction, and the direction is not less wrong or less socially pathogenic because the associated mythology is in part self-validating among those who believe it and act upon it. [his emphasis] (Bateson, 1976, p. 106)¹⁹

Obviously, the differences between Bateson and Haley over the issue of control ran deep. To look at a broader perspective for a moment, it was Haley who did a greater share of therapy where the issue of control and strategies is, of course, more pressing than in theory. Moreover, the structure of reality in the DB framework is conducive to

¹⁹In this vein, see also Bateson, 1976, p. 86.

a power interpretation. If one cannot not comment on a message, and if such comments hold some power, (which they do), then a power interpretation is a consistent interpretation.

Jackson's primary line of development has already been reviewed with regard to the development of the paradigm's relational element. Weakland's main interests are persistently more difficult to discern. He published fewer papers than did the other project members, yet a high proportion of his works were landmarks (especially 1956; 1958 with Jackson; 1960; 1972 with Fry; and 1974). He presented a chameleon-like image, extending Jackson's homeostatic concept (1960), and also Fry's work on third-party and institutional double bind (1962). In 1974, he attempted to tie the DB to a larger framework and cast a perceptive eye on its history. He appears to have been equally facile with therapeutic work, at least toward the later years of the project after dissolution, when he concentrated on brief family treatment.

Organizational schism. Suffice it to say, there were persistent and fundamental differences among project members. Although these differences probably included personality frictions, it is also apparent that these individuals were approaching the project from disparate intellectual traditions. They differed in their assumptions about human nature and motivations, the nature of relationships (e.g., Jackson's position vs. Bateson's vs. Haley's power dynamic), and epistemologies; their view of what constituted important problems differed somewhat: Bateson

stressed abstracted viewpoints, Haley strategic and Jackson experiential.

Under the pressure of empirical contact, the differences in temperament and the commitments to disparate intellectual traditions developed into an organizational schism. In November, 1958, Jackson founded the Mental Research Institute. Jackson (1968, p. V) at one point briefly described MRI's inception:

Our first grant started in March, 1959, and the staff then consisted of myself, Jules Riskin, M.D., Virginia Satir, A.C.S.W., and an inexperienced, frightened secretary. The Bateson project maintained its autonomy, but we had a close working relationship. The MRI operated under the umbrella of the Palo Alto Medical Research Foundation (thanks to Dr. Marcus Krupp) and continued to grow until it split off as an autonomous research foundation with its own administrative staff and board of Directors.

Despite numerous efforts at clarification (Bateson, et al., 1963; Haley, 1976a and 1976b; Jackson, 1968), the two projects and the relationship between them was often confused. It may well be because they shared membership to some extent, and even at one point, the same building. Jackson, 1968, p. V) has said that Haley and Weakland subsequently (after 1962) joined MRI as full-time principal investigators and that Bateson became a research associate, participated in treatment, and served as an informal research consultant. Haley (1976a, p. 92 and 1976b, p. X), however, has indicated that Bateson had no part in the MRI.²⁰ After the 1962 DB dissolution then, Jackson, Haley and Weakland were

²⁰"Although the two groups have been confused with each other, in actuality Mr. Bateson declined to be a member of the Mental Research Institute and did not like his project to be confused with that group." (1976b, footnote, p. X) "...Bateson would not allow his project to be part of the Mental Research Institute and so no personnel or projects were shared." (1976a, footnote, p. 92)

once again working together, albeit on different material. A good account of MRI's approach, particularly with respect to therapy, though of course indirectly relevant to a larger conceptual view, is Satir's Conjoint Family Therapy (1967).

The organizational schism gained expression in late 1958 and early 1959 and was based on fundamental differences in temperament and intellectual framework. MRI has continued to the present time. The DB project continued to function, productively and creatively, from 1959 to 1962, elaborating the paradigmatic elements laid down during the early years. The ability to do so speaks clearly of the ability of the DM members to work concertedly towards certain goals while screening important differences. If nothing else, the DM functioned to allow that collaboration, at the expense, no doubt, of the refinement of individual positions, but with the result that the productive life of the research group was extended by a third.

Haley (1976a, p. 91) has given a clue to the DM validated standards that facilitated the collaboration: that is, he later articulated the "framework of consensus" within which the members could work. To this extent, the DB project remained a coherent research program during its last three years.

It would seem apparent that the disagreements within the project appeared when there was an attempt to look at real people communicating real messages. At the more abstract level, there was surprising agreement. All project members believed the best approach was in terms of some theory of systems with an emphasis upon rules and patterns and upon stability and instability over time. All project members

approached the problems in terms of levels: levels of messages, levels of rules, levels of governing process. Additionally, all considered it more and more obvious that the schizophrenic's behavior was in some way adaptive to the particular kind of family organization in which he was raised. It was also assumed by all that family members responded in error-activated ways to each other, and that the governing process should be described in an hierarchical way. (Haley, 1976a, p. 91)

The list of consensual points reviewed by Haley reads like a summary of the paradigm's characteristics. There are Russell's levels, the homeostatic mechanisms, and the reconceptualization of schizophrenia as adaptive and meaningful.

That this set of consensual beliefs facilitated the continued existence of the DM is obvious. The schism occurred when the DB had turned from almost exclusively abstract work to more empirical concerns, particularly with respect to the issues of family organization and family therapy. The consensual beliefs allowed them to pursue these issues in concert.

Second shift: family therapy. With the attempt to describe family organization, the DB project began to think about changing it. It is very clear this was a derivative goal, as they had originally had no plans for family treatment (Jackson and Weakland, 1961). Haley (1972, p. 114) portrays the shift as the idea occurring to them that the way to change schizophrenia would naturally be to change family organizations. This occurred in approximately early 1957.²¹ Later in the year they

²¹The project began seeing families with a diagnosed schizophrenic family member in 1956, but Jackson at one point (1961, p. 272) indicates

discovered that several other people had also begun to try and change families (Haley, 1972, p. 115), as family-oriented clinicians rather than in collaborative individual therapies.²² In fact, Jackson and Weakland, writing in 1961, state, that though family treatment was a growing trend, there was still only a limited amount of such work being done, and even less published.

In 1958, they applied for a grant to change families, and began to further reconceptualize adolescent (and early adult) schizophrenia as an outcome of difficulties in disengaging from the family (Haley, 1972, pp. 115 and 116). During their efforts to change families, the DB DM developed a clinical therapy and theory that was both consistent with the paradigm and DM characteristics and was easily differentiated from other, later forms of family therapy developed around different paradigms.

For instance, they point out that as a clinician moves from individual work to family treatment, s/he will become more active, less interested in psychiatric diagnoses or dynamic formulations and more interested in describing the identified patient in terms of an interlocking milieu in a social or subcultural context; s/he will also

that just prior to joining the project he had been seeing patients and parents together. The patients in this case, however, were neurotic and autistic children, not schizophrenic adults. (See Jackson, 1965, p. 2 footnote)

²²In fact, I. Boszormenyi-Nagy had begun his experimental family treatment unit for schizophrenic women at Eastern Pennsylvania Psychiatric Institute in 1956, and Bell and Ackerman had been seeing families.

increase the number of people in the treatment (per case) (Jackson and Weakland, 1961, p. 31).

A body of technique and theory was developed, with the emphasis on the structure of the family and the interpersonal communications, not the content. Thus, a well-tested first session gambit is to assemble the family, ask them to pick one of their problems and negotiate a solution. The family therapist then listens, not for the content of their process, but for the structure--the characteristic sequences of interaction that occur. These can take a variety of forms: individuals blaming each other "or a child [who raises] his demands each time the parents are about to agree, or one person [who "invalidates"] another's perceptions." (First, 1975, p. 9). The structure of these interactions become the focus of the change efforts.

A favorite DB change technique is "reframing" or "relabeling," which by changing the context of a behavior, changes its meaning, and thus often changes people's behavior. Thus, behaviors of the identified patient are relabeled as normal, as rational tactics, to look positive if possible (First, 1975). At times "mad" behavior is reframed as "bad", or vice versa; either shift in direction can take the identified patient out of pathological position. First (1975) points out some spectacular examples of reframing.

Probably the boldest example of this was the husband who chased his wife with an axe, and was told he was trying to get close to her. Candide was clearly a talented relabeler, and so too was Tom Sawyer, whose "reframing" of the task of whitewashing a fence is quoted as exemplary

by the authors of Change. Here are some examples of re-labeling in practice:

1. The family of an anorectic is asked, "How long have you had this fasting problem?" (First, 1975, p. 9)

Other techniques were frankly irrational:

Don Jackson, asked how he might try to produce insight, if he ever wanted to, replied, "I would tell them to treat [this] daughter the way they are already treating her, and let them discover how they're treating her. In other words, try to produce a "runaway" in the system. That would produce more "insight" than just telling them what I think they're doing... (First, 1975, p. 9)

At times, the irrationality could culminate in a full-blown therapeutic double bind, such as prescribing the symptom.

Accompanying, or following, the efforts at therapy, were developments in family theory, e.g., Bateson (1961, p. 139) pointing out that when they attempted to determine the location of control in schizophrenic families, they found "something very peculiar--that control and responsibility are not located in the same person."

Several issues emerged during the development of their family therapy. Weakland directed his attention to a particularly behavioral concern, one consonant with the communicational aspect at a technical level, and to the relational at a meta-theoretical one.

This increasingly appears as the most important question in family therapy, or even for schizophrenia or psychopathology quite generally. Unless one is to fall back on some idea that people by fundamental nature are oriented toward disease, so that pathology is inherently self-sustaining, rather than "normality is normal," the central issue is not the question of the original root causes of schizophrenia. It is not even the question of what sort of present family interaction leads to schizophrenic symptoms

in one member. Instead, the central issue concerns the nature of the maintenance of family systems involving a case of schizophrenia. What in the family interaction makes for the fundamental stability and persistence of these family systems that is so striking in the face of the general dissatisfaction and unhappiness of the members, their stated desires for change, and often the best efforts of a therapist? (Weakland, 1962, p. 68; his emphasis)

Also of theoretical concern was the effect of family therapy on a variety of areas; the prognosis of the identified patient, other family members, long-term change. The issue of therapeutic efficacy was repeatedly addressed (Haley, 1976a and 1976b; Jackson and Weakland, 1961, pp. 34-35; Weakland, 1962). Also, the shift the DM implied, from disease and medicine to the social sciences, was of particular interest to Jackson; he and Satir pointed to some developing family concepts they thought particularly promising, including: "family homeostasis, coalitions within the family and their stability, role-playing, acquisition of family models, three-generation theory, the theoretical applications of the game theory, decision-making [and] recognition of resemblance..." (Jackson and Satir, 1961, p. 46).

Another, less insistent, concern was the nature of change. The DB notion of change, according to First (1975) originated in Bateson's work with Russellian levels and levels of learning.

As I trace it, this notion of change first appears, innocently enough, in Bateson's thoughts about a porpoise: the experimenters were trying to teach the porpoise to produce new behavior. Each time it did something new accidentally, they would reward it. The porpoise would begin each show by displaying its latest "new" trick, but it wouldn't get a reward unless it did something still newer, by chance. The porpoise grew understandably moody.

But finally it caught on: it went into a state of creative agitation, and at its next performance produced four pieces of behavior never before observed in the species. It had learned to learn.

This corresponds with the cybernetic notion of change. There is "first order" change: this is merely substituting one item of data (or behavior) for another. Then there is second order change: this is a change in the kind of operation that is carried out--"a change in its [the computer's] way of behaving." Second order change means a shift to a "meta-level" of programming.

This second-order change corresponds to Bateson's "deutero-learning." From accounts, it would appear that families treated with DB therapy would be induced into first-order changes, but ironically, not second-order?

These theoretical concerns continued to be articulated throughout the development of their mode of family therapy until the DMs dissolution, and to a lesser extent by some members, afterwards.

DM dissolution in 1962. Word reached the professional public of the project's dissolution in Family Process.

Gregory Bateson is disbanding his research group after nine years of research on the nature of communication. His Family Therapy grant terminates in August of this year and he plans to release his associates. He will then prepare a book on the project work and study the metacommunicative behavior of the octopus. (1962, p. 134)

At the time of dissolution Bateson had remained Director, Haley and Weakland were Research Associates, Jackson and William Fry were Consultants (Haley, 1962, p. 69 footnote). Haley later (1972, p. 117) stated that Bateson had plans to study communicational phenomena in

dolphins, Weakland studied Chinese art and Haley joined Jackson at MRI. In 1967 Haley went to the Philadelphia Child Guidance Clinic, to his and Salvador Minuchin's mutual benefit.

Bateson (1976, p. 105) aptly considered the ten year project "enormously and nontrivially productive." He, as usual, was quite correct. In ten years time, they produced: a revolutionary paradigm for a mysterious disorder; a productive DM that elaborated the paradigm itself, extended it to new areas and shifted focus twice (to family organization, then therapy); a clinical practice--one of the forms of family therapy--with its theory, techniques and rationales; and a body of related work that fills out the framework. In addition, MRI was established and continues its work to the time of this writing. Also, Jay Haley founded Family Process, the first journal for clinical family concerns; he served as its editor, from 1962-1969. The DB project publically reconceptualized a human psychological disorder and helped to open up the family therapy field as a field (along with such other "pioneer-types" as Ackerman, Boszormenyi-Nagy, Whitaker, and Bowen). This opened up family work in schizophrenia as both a content area and a reconceptualization of the disorder. Their reconceptualization, interestingly, did the same thing for schizophrenia that Freud had done for the neuroses: defined the behaviors as adaptive, and meaningful, then provided a revolutionary reconceptualization.

Formal DM characteristics. Another important point is the congruence

between DM development in the DB project and Kuhnian DM characteristics. It is obvious, first of all, that the DB project had a paradigm, in fact a revolutionary one, and a coherent research structure, as well as a constellation of group commitments--those consensual points, especially, that proved so useful from 1959 to 1962. That they shared a set of "scientific habits--including intellectual, verbal, mechanical and/or technological" is also the case, except for "intellectual." There, the members clearly differed and differences in these intellectual habits developed into an institutional split. Also, there was only the relative unanimity that Kuhn and Masterman so practically qualified, in problem-choice, adequate solution and communication.

The project met the criteria of internal DM structure. It had heuristic models--levels or hierarchies and Logical Types, as well as closed-system homeostatic devices. Its symbolic generalizations included such terms as: message and meta-message, reality markers, double bind, therapeutic bind, incongruence, identified patient, and reframing. Its values included: the positive valuation of efficacy in treatment, and a bent toward problem-solving; delivering care to as many as possible as quickly as feasible; as well as the ambivalence in values between science and clinic work, complexity and purity with triviality. It even had the prescience to provide instrumentation; the project made extensive, and early, use of video-taping in clinical work. Finally, it provided theories for both schizophrenia and clinical practice.

CHAPTER VII

EVALUATION OF THE USEFULNESS OF A KUHNIAN ANALYSIS FOR THE EMERGENCE OF FAMILY THERAPY

During the preceding chapters, I have used a modified Kuhnian analysis to elucidate and interpret the emergence of family therapy. It is now necessary to evaluate this interpretation with respect to the emergence of family therapy, the efficacy of this form of modified Kuhnian analysis and, in Chapter VIII, the implications and applicability of such an analysis to the contemporary felt crisis in psychology.

Evaluation of a Modified Kuhnian Interpretation of the Shift to Family Therapy

It is my contention that a modified Kuhnian analysis allows an informative interpretation of events preceding and co-existent with the emergence of family therapy, and that the analysis provides a useful interpretation of at least one family therapy approach, the DB, from inception through formal dissolution. Such an analysis allows us to interpret some events in a different manner than previously, and occasionally, to find sense in what had appeared to be random processes.

This analysis has identified and interpreted certain problems in classical psychoanalysis as related to the emergence of family therapy. This set of problems --- the anomalies were obdurate discrepancies between framework-generated expectations and empirical findings. Though psychoanalysis encountered a variety of problems, not all were anomalies

nor were they related to the emergence of family therapy. The analysis permits the differentiation of "anomalies" from "problems as a general class," the identifies those which were related to the 1950's family therapy shift.

Similarly, the modified Kuhnian analysis emphasizes out of all the controversy swirling about psychoanalysis, certain controversies during the early and middle 1950's as related to the appearance of family therapy during the latter part of this period. A Kuhnian interpretation allows us to view these controversies as constituents of a "felt crisis" during the first part of the 1950's in classical psychoanalysis. The interpretation highlights the presence of controversies, the re-examination of fundamentals and the awareness by the people involved that there existed a crisis. Although the question of parameters in psychoanalysis is often regarded as important, only within a Kuhnian interpretation is it related to the emergence of family therapy and is it identified as a bona fide crisis for classical analysis. Moreover, a Kuhnian analysis provides an interpretation for the emergence of family therapy in the 1950's; with the emergence of anomalies in the 1930's and 1940's and crisis in the early 1950's, the Kuhnians would interpret this sequence as probably evolving toward the emergence of a new paradigm, and would predict this paradigmatic emergence as occurring soon after the awareness of felt crisis. Previously, the timing of the emergence of family therapy had remained obscure, even to the participants; Halye, for example, speaks to this issue:

Then for unexplained reasons a number of therapists began to deal with whole families in the 1950's often without knowing that anyone else was doing so. Many of these people did not write for professional journals or attend meetings, so their work was known only locally, if at all. Curiously a decade later many experienced family therapists still had not met each other. If they had been introduced, they still had not sat down together to discuss their work and seek a common view on what changing a family is all about.

This movement toward therapy with whole families occurred just when the dynamic concept of the individual, and psychoanalytic treatment, had won power and prestige in the psychiatric establishment after a long struggle. Everyone who was respectable wished to practice psychoanalysis or at least to give psychoanalytically oriented treatment. (Haley, 1971b, p. 2; emphasis added)

The Kuhnian interpretation places the emergence of these nearly simultaneous family therapy groups within an intellectual and historical framework. The analysis identifies the inception of at least one of these approaches, the DB, as a revolutionary paradigm. The "paradigm" concept expresses the combined conceptual and technical power of an innovation like the DB hypothesis. A Kuhnian interpretation helps to explain not only why such a "paradigm" becomes so influential (it answers questions, solves problems and attracts adherents) but how (through elaboration by normal the science activity of adherents into a DM). The paradigm concept also provides a crucial perspective by which to differentiate the DB family approach from the classical psychoanalytic (and also the relational); the paradigm in these clinical approaches depended upon mechanisms of change and the techniques by which to gain such change. Their criterion serves to differentiate approaches more

reliably than disciplinary name, or the training and credentials of therapists.

A Kuhnian analysis using the "paradigm" and "DM" concepts allows an interpretation of the DB group's work as the elaboration and protection/nuturance of the paradigm. This analysis allows a non-intuitive interpretation of why the DM was able to function from 1959-1962 despite its institutional division; the structure and functions of the "DM" provided the basis for sufficient consensus, and the mechanisms by which divisive concerns were subdued, so that productive work on the paradigm could continue.

The Kuhnian analysis, however, cannot explain why the DM underwent this institutional split. (An attempt will be made to do so in Chapter VIII).

A Kuhnian analysis allows an interpretation about the relationship of periods of fundamental innovation in science, with periods of accretionary progress, highlighting the dialectical relationship between them, and implying a model of scientific development more complicated than the linear accretion of facts. It is particularly interesting to note that though normal science is "usually considered the real, or usual, (or 'normal')", a DM both begins and end in revolutionary science.

Finally, it should be noted that members involved in the same processes for which the Kuhnian schema was designed, have found it helpful in interpreting their own activities to themselves. At least some of the DB adherents, for example, have late in their work referred

to Kuhnian concepts in relation to their own work (Watzlawick and Weakland, 1977; Weakland, 1974).

Controversies

A modified Kuhnian analysis of long-standing controversies can provide interpretations particularly relevant to debates between the adherents of different approaches, i.e., in Kuhnian terms, DMs. The analyses allows us to interpret certain of the controversies involving DB members, interpretationists and relationists. (As this interpretation has not, to my knowledge, been done previously, it will be explicated in rather more detail than the preceding section).

There were a number of controversies which were carried out in methodological terms, but which upon examination with the Kuhnian schema, are more productively interpreted as debates between DMs; that is, they are only ostensibly methodological. If this type of controversy continues to be debated at the methodological level, it is unresolvable. If it is debated at the DM level, it is at least potentially resolvable. One such long-standing, sterile controversy took place between Kurt Eissler, an interpretationist, and Freida Fromm-Reichmann, a relational analyst.

Ostensible methodological controversy between the interpretationists and the relationists. A good deal of the disagreement between these two groups took the form of ostensible methodological debates. To

illustrate, a segment of the Eissler-Fromm-Reichmann ostensible methodological controversy will be reviewed, with comment pointing to the implicit DM vs. DM quality allowed by a Kuhnian interpretation.

Over a period of several years, Eissler and Fromm-Reichmann argued in the literature, Fromm-Reichmann emphasizing the techniques which fostered the therapeutic reality relationship and Eissler concentrating on the crucial role of interpretation. Occasionally, Fromm-Reichmann would take a swipe at interpretation (and Eissler), while he would more than occasionally criticize her technical innovations, rarely confronting directly the differences in mechanisms of change. Also, Eissler had a number of colleagues, also interpretationists, who took up the debate; their comments and positions on these subjects will be included with Eissler's, as will some small examples of true, intra-DM debates. These intra-DM disagreements are actual methodological or semantic controversies (not ostensible) and thus, argument at that level can produce resolution; these will be included to provide contrast with the inter-DM debates.

Fromm-Reichmann's position. In 1943, Fromm-Reichmann discussed "technical requirements" or modifications to classical psychoanalysis, which had gradually developed during clinical practice with psychotics. Since the ostensible methodological controversies largely involved these seven "technical requirements", they will all be reviewed here. As will become clear, many of the modifications were directed towards

facilitating the therapeutic reality relationship.

The first technical modification concerned "the couch." It was Fromm-Reichmann's contention that the "couch regulation is neither understood nor followed by the psychotic patient" (p. 133), and sitting behind the patient in the beginning of treatment was contraindicated as it fostered unreality, the therapist serving a bridge to reality for the psychotic patient.

Moreover, depending upon life-histories and habits of neurotics, it may or may not be appropriate to lie on the couch and so, she recommended any position that allowed patient and analyst to look at each other whenever the patient wished. Seated behind the patient, either participant may mentally "wander away" from the interpersonal relationship.

Fromm-Reichmann (1943, p. 133) also elaborated on the analyst's position, which later drew fire from Eissler.

Freud remarked that he could not endure to have patients gazing at him for eight hours. This suggests a change in the eight-hour system rather than the maintenance of invisibility for those who share Freud's feelings. Personally, I have found a ten- or fifteen-minute interval between interviews most helpful.

Fromm-Reichmann's second technical modification addressed what she felt to be the rote, unspontaneous, going-through-the-motions quality which at times was found in the free association pattern, where for the most part the patient talked and analyst listened. She felt that such an attitude at times masked the analyst's personal timidity. The

"What's the use?" quality on the part of the patient was felt to require "the attention of a psychiatrist who is careful to show that he is genuinely concerned with the patient's welfare and that he is methodically trying to re-establish the lost spontaneity of the patient in active interaction, strictly within the reference frame of doctor-patient relations. " (1943, p. 134)

Fromm-Reichmann's third technical modification was the abandonment of free association; she felt that it was "quite unnecessary" to encourage free association with psychotic patients as they exhibited, without probing or prompting, the material that the technique of free association was used to elicit. She elaborated, moreover, that with increased experience, insight and skill, it was often possible to proceed to the same goals "by an utterly unconventional, direct, and precise questioning." (p. 134)

Fromm-Reichman also abandoned the technique of interpretation, which, it soon will be seen, was crucial to the practice of psychoanalysis to Eissler and other interpretationists. Fromm-Reichmann felt that psychotic patients were able to understand their verbal productions far more clearly than the analyst, and as such, Fromm-Reichmann felt it to be "crudely redundant for the psychoanalyst to explain what he believes he has understood." (p. 134) Rather, an appropriate response would be indicated. At the time of writing (1943), she felt that interpretation of content had largely been abandoned and interpretations regarding transference, resistances, and defenses had been reduced. Fromm-

Reichmann felt, along with Freud, that because of access to psychoanalytic insights, "modern patients" had less need for the insight-provoking interpretations. Further, there was a danger of inducing what she termed "anti-therapeutic self-consciousness by excessive or untimely interpretations" (p. 135), a process she felt had not been sufficiently attended to in the literature. Only dynamic and interpersonal processes immediately related to the etiology of a psychosis required interpretation (p. 135); if I understand this correctly, the interpretations would provide associations between relational dynamics and/or events and the development of psychosis. Precipitating factors would be very important in this form of clinical work. It appears that the form of interpretation referred to here has to do more with relational than intrapsychic dynamics, though both produce insight. In the relational DM, however, insight did not occupy the same central role in the change mechanism.

Fromm-Reichmann's fifth modification concerned etiology, and particularly "repressed content." She felt that the contents of repression were not "all sexual in nature nor all due to hostility, as advocated for awhile by some psychoanalysts." (p. 135) Rather, all emotions, thoughts, impulses, etc., which the patient had experienced regarding significant people in his life can become pathogenic and the contents of repression, if they are incompatible with the patient's private standards--learning from the social standards of significant persons in his/her life. "It is not the biological aspect of sexuality but the pathological features

of their interpersonal relations which more frequently create sexual problems." (1943, p. 135) Thus, the etiological classical sequence is stood on its head.

Fromm-Reichmann's sixth change in technique was to "allow" acting-out impulses to some extent. Acting-out was regarded as a not uncommon necessary preliminary to verbal expression; the latter should never be forced, as any beginning rapport might be destroyed and preclude the possibility of further treatment. In light of this, Fromm-Reichmann questioned whether analysts should continue, in all cases, to suppress acting-out by neurotic patients during interviews, and whether this suppression was always directed to furtherance of therapeutic aims or to fear of what the patients would do if the acting-out were permitted.

Fromm-Reichmann's last major modification in technique involved that the therapist examine his/her value system and be aware that it inevitably influenced the treatment of the patient; psychotherapy, according to her, was not a value-free enterprise and could not be. "Psychoanalysts pretend in vain that their values are irrelevant in therapy or influentially non-existent in the psychotherapist. There are legitimate values for every psychoanalyst." (p. 136) In particular, Fromm-Reichmann stressed examination of the degree of conventionality held by each analyst; she felt that for psychotic patients, particularly schizophrenics, recovery did occur, within somewhat unconventional expectations. (p. 136) The analyst whose values included a marked conventional set was often

disappointed in the adjustment of former patients, and by establishing these somewhat arbitrary guidelines, at times impeded that very adjustment.

Five years later, Fromm-Reichmann presented the changed engendered by the relationalists as "changes in the technique of psychoanalytic treatment during recent years [with] regard to both the establishment of the doctor-patient relationship and the approach to the contents of psychotic communication." (1948, p. 164; emphasis added). By now, with the assistance of the paradigm and DM concepts, it's clear that these modifications were far more than only methodological or technical disagreement. The differences in the doctor-patient relationship were at the heart of the relationalist position; the reality relationship, not the transference, was held to be both contextual and curative, and as such, constituted a different mechanism of change, and therefore, a different paradigm. Similarly, the differential treatment of verbal content, with the relationalists eschewing interpretation, is related to the relationalist paradigm and is an abandonment of the psychoanalysts' therapeutic mechanism of change. Interpretation was held to be unnecessary (as the problem was a surfeit of insight rather than too little in psychosis), and "inadvisable if not much of the time redundant." (Fromm-Reichmann, 1948, p. 167) With the abandonment of interpretation, and the change in the doctor-patient relationship (from transference to reality-based), Fromm-Reichmann was clearly committed to a different paradigm and DM, the Sullivanian interpersonal. With a Kuhnian analysis,

some of the controversy between Fromm-Reichmann and Eissler can be seen as controversy between two different DMs.

In 1948, Fromm-Reichmann defended the reality relational element from one of Eissler's criticism, adopting a technical stance,

Non-professional closeness, pretense of personal friendship, and violation of the schizophrenic's fear of closeness, with its concomitant fear of his own hostility, were, of course, avoided. Also omitted were such signs of acceptance and permissive gestures as would go beyond the psychiatrist's endurance to sustain or repeat over a prolonged period of time. This had to be seriously considered, lest what appeared to be therapeutic acceptance would ultimately be reversed into a new case of rejection.

In spite of this background of basic permissiveness, treatment was not just effective by virtue of the "love" offered, as Kurt Eissler has intimated. (p. 165)¹

Fromm-Reichmann here, argued in terms of technical points in clinical practice to avoid while establishing a therapeutic reality relationship. She did not deal with any of those interrelated commitment aspects of scientific practice now termed as DM and she certainly could not have talked in terms of differing paradigms.

Eissler's position. In 1953, Eissler responded to some of Fromm-Reichmann's technical modifications. Her modifications, particularly with regard to the use of the couch, provoked his ire; for example (1953, p. 106):

¹Kurt R. Eissler, "Limitations to the psychotherapy of schizophrenia," Psychiatry, 1943, VI, pp. 381-391.

Freud reported some of the subjective factors which influenced the evolving of his technique. For example, in explaining his request that the patient take the supine position during analysis, Freud...mentions his dislike of being stared at for several hours and he goes on to add other reasons which make the supine position preferable.

When she discussed her deviation from classical psychoanalysis, analyzing in a face-to-face situation, Freida Fromm-Reichmann...supported her argument by quoting Freud's idiosyncrasy. Her argument is out of place. An analyst may be an exhibitionist, and may therefore prefer a face-to-face technique. Whatever technique a therapist may devise can be used in the service of his pleasure principle. The value of a technical measure must rest on objective factors. If it coincides with the therapist's pleasure all the better, but this coincidence is not a decisive factor in judging and evaluating the given technique.

Eissler continued in this fashion regarding Freud's reasons for instituting the couch and supine position. Besides the slightly inappropriate air when attributing the face-to-face seated position to the therapist's possible exhibitionism, his rejoinder in the debate is certainly strong, and put in technical terms. With so many fundamental points of disagreement between them, representing different DMs and ranging through mechanisms of change, diagnostic categories appropriate for treatment, mentors, and roles and approaches of the therapist, Eissler waxes wroth about the couch, a relatively minor point of technique. Neither Eissler nor Fromm-Reichmann discussed the differences between the respective mechanisms of change, or conceptualization of disorder; they skirted the fundamental differences differentiating their DMs, and debated about relatively minor technical points as though they were

independent of their respective frameworks.

This is an example of an ostensible methodological controversy that can more helpfully be regarded as expressing inter-DM differences. Obviously, the techniques were embedded each in their own DMs, and the differences in technique expressed (at least in part) differences between the DMs. When the debates revolved around only these technical differences, no resolution was possible for several reasons. First, each debator was hearing the other's technical argument in terms of his/her own DM, and thus it probably, and necessarily, made little sense. Second, the technical differences were an expression of several interrelated points of difference, so that continuing to debate only on the technical level continued to obscure the other points of difference, impeding their clarification and possible resolution.

At a different point, Eissler (1958, p. 222) disagreed with Loewenstein regarding use of the couch

...whatever Dr. Loewenstein's final decision regarding terminology may be, I think we ought to distinguish strictly between variables and constants within the classical technique. I would count the patients' recumbent position as a constant and not as a variable--as Dr. Loewenstein does at one point.

This small debate, contrary to the above, is a valid, resolvable intra-DM methodological debate. As both Eissler and Loewenstein shared an interpretationist psychoanalytic DM, a disagreement about a technical point could be debated with recourse to the same network of commitments, paradigms, meaning sets and goals. The crucial difference

is whether the participants share a DM, or are attempting to settle a technical point across DM boundaries.

Addressing a more important point, Eissler (1953, p. 136) considered the lack of distance or perspective, or rather, the lack of ability in schizophrenics to differentiate between the possible and the real at times.

This incapacity to lift himself out of the context of phenomena at one point at least must make the technique of treating schizophrenics essentially different from that of neurotics if one extends the treatment to the treatment of the ego modification. It is strange to notice this technical problem which is most typical of the treatment of schizophrenics is barely mentioned in the contemporary literature on the psychotherapy of schizophrenia.

In a footnote, Eissler (p. 136) continued:

Fromm-Reichmann seems to claim that there is essentially no difference between the technique of treatment of schizophrenics and neurotics, a point of view which in my opinion is tenable only if the field of therapeutic action is limited to the patient's interpersonal relationships with disregard of the patient's ego modification. (emphasis added)

Obviously, Eissler was addressing the two differential goals of treatment for the respective DMs under the guise of "technique of treatment." In point of fact, that "technical problem" was rarely mentioned in the contemporary literature because it was not a technical issue. If an analyst dealt only with patients of basically reliable, or unmodified, ego, s/he was not seeing schizophrenics and the issue for him or her was largely irrelevant. If an analyst were treating schizophrenics, the classical objections regarding the treatment of modified

egos generally held little salience for the therapist; this was the case partially because the therapist would very probably not have embarked on treating schizophrenics if the classical proscriptions had been salient. Moreover, if treating schizophrenics, the therapist was likely to be heavily influenced by the Sullivanian interpersonal framework, and thus would have had little use for a concept like the modified ego. The schizophrenic patient's lack of distance on his/her phenomenology was not the technical issue as presented by Eissler, because it was not an issue for those treating or those not treating schizophrenia. The inability to achieve distance was not a technical issue for the relationists and was an issue only for the interpretationists, who most often did not treat this sort of patient in the first place.

The basically reliable ego, or, the unmodified ego, was a central concern for Eissler's and the other interpretationists' approach. Interpretation was efficacious only with the basically reliable ego, both theoretically and in fact.

The set of technical arguments regarding the "basically unreliable ego" or the "modified ego" is largely an ostensible methodological controversy about DM differences in subject, goal and change mechanism of treatment; the latter were rarely, if ever, discussed at their appropriate level of discourse. Their expression was usually limited to the technical level.

For instance, according to Eissler (1953, p. 116), the legitimate clinical population was comprised of those individuals for whom, despite

"their symptomatology, the ego had not been noticeably modified..." by trauma or whatever. They were appropriate since the "basic model technique" could be used "without emendations."

In other words, if the ego has preserved its integrity, it will make maximum use of the support it receives from the analyst in the form of interpretations. The exclusive technical problem in such instances is simply to find that interpretation which will provide the ego, in the restitutive phases of the treatment, with maximum support. (1953, p. 116; emphasis added)

Such an ego would be able to enter into the therapeutic work. The patient's ego would be sufficiently strong to work towards recovery and the tool with which the analyst can accomplish this recovery is interpretation. With the reliable ego, "The problem...is only when and what to interpret; for in the ideal case the analyst's activity is limited to interpretation; no other tool becomes necessary." (Eissler, 1953, p. 108) Eissler then considered the modified ego, and ruled it out of DM activity (in a process similar to the DB DM's ruling out phenomenology as metaphysical).²

At the end of the scale is the ego of the psychotic, with whom the analytic compact is impossible. There is scarcely anything to say about this end of the scale...(1953, p. 122)

Turning to schizoprenias, where the ego modification is most obvious, Eissler stated that the most remarkable difference in treatment

²This is not to imply an analogy of content, merely a parallel in process; this process was discussed in Chapter 1.

concerned the "essentially different technique of handling the transference" (p. 134). Specifically, he felt that with unmodified egos, transference developed spontaneously, whereas with the schizophrenics it had to be produced. The technique of free association could not be used as the patient would probably be incapable of cooperating and the technique might precipitate "regression" in any case. Lastly, interpretation was "thrown out of gear" and did not convey insight to the patient. (1953, p. 113)

Eissler's reasons for discouraging the psychoanalytic treatment of modified egos were perfectly appropriate of course. Within the framework of his DM, his DM arguments were logical, meaningful and helpful; they can be construed as "true." For instance, free association often did produce "regression" (or a recurrence of the psychosis) in schizophrenic patients; similarly, interpretation was not efficacious in clinical practice with this population. It is perhaps a difficult point to appreciate how "correct" Eissler was within his DM and how completely besides the point his arguments were for those adherents to other DMs who were more successfully treating schizophrenic patients.

Yet, from within his DM, Eissler continued to pose their differences in methodological terms (as did many of the proponents). Thus, he defended the role of interpretation and its eventual goal, the structural change of the ego, from recent pressures (1953, p. 126; emphasis his).

It is well known that the proper use of interpretation is

difficult and complicated. But so central is this tool that any proposed variation or addition should be scrutinized with the greatest care. The introduction of parameters, even of such simple ones as are necessary in some cases of phobia, contains dangers which must not be overlooked. Each parameter increases the possibility that the therapeutic process may be falsified, inasmuch as it may offer the patient's ego the possibility of substituting obedience for a structural change.

The term obedience, not entirely an accurate one, is used here to designate all those improvements which a patient may show under the pressure of the therapy but which are not based on a dissolution of the corresponding conflicts. A patient often prefers to produce adjusted behavior instead of a structural change.

Moreover, he later implied some deficits in the skill of those therapists who introduced modifications into the classical interpretative technique (1953, p. 127)

Again, this paper is not the place for a discussion of what a proper interpretative technique is; it is mandatory, however, that a warning be raised against the quick introduction of parameters under the justification that interpretations have been of no avail. There is a great temptation to cover up, by the introduction of parameters, one's own inability to use properly the interpretative technique.

The issue of expediency particularly disturbed him. Eissler at several points (1953, pp. 113, 125, 126, 127) felt that expedience was often followed, rather than a stricter course dictated by theory.

Remembering that the goal of interpretationist DM is ego reconstruction (rather than the disappearance of symptoms or changes in interpersonal relationships), both the DM meaning of Eissler's next comment and the ostensible technical quality are obvious.

The content of this footnote is of formidable importance... Technical innovations are introduced in large number and are supported by the simple-minded justification that the innovator has noticed subsequent disappearance of symptoms. The question of "at what cost to and limitation of the ego" is no longer asked; instead pride at the alleged superiority of the contemporary analyst's knowledge, indicates many authors believe that Freud's safeguards against the effect of the therapist's personality--in situations where a structural change, induced by the analytical process, ought to take place---have become superfluous. (Eissler, 1953, p. 113, footnote)

Other interpretationists. Eissler was not alone among the interpretationists who were concerned about the relationalist modifications in treatment. With the Kuhnian analysis at our disposal, it is possible to interpret their differences as fundamental because of the different paradigms; at the time, however, the controversies were couched in terms of technique, though the vehemence of the debate partially expresses the realization of the participants that a good deal was at stake.

For instance, Stone (1954, p. 567) takes a position similar to Eissler's, regarding the reliable ego, and the modifications in technique and goals:

We would, while acknowledging that other psychotherapeutic agents play an important role in the psychoanalytic process, assign to interpretation the unique and distinctive place in its ultimate therapeutic effect. We would, I think, require that the interpretations achieve this effect through the communication of awareness of facts about himself to the patient, with the sense of emotional reality that comes only with technically correct preparation, rather than through certain other possible effects in the transference counter-transference system, which occur so frequently in other psychotherapies. (Certainly, they occur also in psychoanalysis, but they are regarded as miscarriages of effort.) (Stone, 1954, p. 574; emphasis added)

Stone's comments about the "transference-countertransference system" in this context obviously refer to what the relationalists would call the "therapeutic reality relationship," which they regarded as essential to recovery and not as miscarriages of effort." This sort of debate is obviously an inter-DM ostensible methodological controversy, and just as obviously, is not likely to be resolved at this methodological level.

In 1958, the papers were published from a symposium on the widening scope of psychoanalysis and the need to adequately differentiate classical analysis from analysis with certain variations in technique arising from the exigencies of treating unreliable egos. The symposium used Eissler's 1953 paper on parameters as their starting point. The panel's task was to help differentiate among "variations of technique which in no way conflict with the basic rules and goals, modifications which may be necessary but temporary interruptions of our procedures and aims, or deviations which lead to a permanent change in the psycho in the psychoanalytic method with a consequent renunciation of its results." (Green-son, 1958, p. 200; his emphasis) This symposium can be interpreted to be a response of concerned interpretationists to the increasingly influential (and adherent-attracting) relationalist group. Green-son, Eissler, Loewenstein and Stone, among others, participated. That there were fundamental agreements among them was recognized by the participants themselves (see, e.g., Eissler, 1958, pp. 223 and 227; Green-son, 1958, p. 200; Loewenstein, 1958b, p. 241) They agreed on the inadvisability

of treating the basically unreliable ego and the need to restrain the treatment within the classical interpretationist lines; though they admitted that at times technical modifications were necessary, they all felt strongly this should be as infrequent and narrow as possible and should in no way alter the classical re-structuring of the ego through interpretation and working through. They were unable to address directly the pressures from the relationalists in terms of paradigmatic issues. Instead, Eissler, et al., became increasingly concerned with classifying any "deviation" from classical technique and controlling its effect in treatment, particularly with respect to interpretation.

In this symposium, several definitions and explanations of the classical approach were made (e.g., Greenson, p. 201; Loewenstein, pp. 202 and 205), all stressing the role of interpretation. (Loewenstein, 1958a, in fact, went on to elaborate the various aspects and types of interpretation (pp. 207-208). The symposium expressed both their awareness of the necessity of in some way addressing the new clinical populations with their non-classical treatments, yet also their commitments to the classical approach. This duality is expressed in the form of the presentations: an opening statement explaining the classical position, consideration of a new clinical population (usually schizophrenia and very occasionally delinquency), reiteration of the classical approach with some small modification to accommodate the non-ideal patient. Very clear boundaries were erected around these small modifications and they were portrayed as unfortunately necessary at times, always dubious and

never as treatment; rather these modifications were designed to make treatment possible. The modifications were always technical and did not address the important inter-DM differences relating to mechanisms of change or role of the therapist.

The resurgence of interest in classical techniques at that particular time is not surprising if a Kuhnian analysis is used. The 1958 symposium was a gathering of respected and concerned interpretationists, coming together to thrash out a response to what must have seemed like misguided, quick and easy, personalized therapy. Simultaneously, the symposium was obviously designed to meet the challenge and put it to rest by establishing those conditions under which modifications were necessary, and those rules under which modifications of technique could be made, yet still be classical analysis. In the course of the efforts, the symposium members engaged in a number of inter-DM ostensible methodological controversies with respect to differences with the relationalist DM; they also engaged in some valid intra-DM methodological or semantic debates among themselves that were resolvable when argued on that level.

Intra-DM solvable debates. Eissler conceded that at times, "parameters" might have to be introduced and gave four criteria under which they should operate. Briefly, a parameter was a modification of technique introduced only if the basic model did not suffice; it must "never transgress the unavoidable minimum, must lead to its own self-elimination" (1953, p. 111) and the parameters' effects on the transference must never be such that they could not be abolished by interpretation

(1953, p. 113).

Stone (1954, p. 576) engaged in a small intra-DM technical debate with Eissler's fourth point.

In an ideal sense, I think the requirements for acceptability of parameters given by Eissler, with one exception, are excellent. The exception is the one which requires that the parameter must terminate before the end of analysis--a requirement which, as the author states, automatically excludes the time-limitation parameter, which Freud used with the Wolf Man.

Stone felt that the fourth criterion was "altogether too severe" and that if the usual conditions of a classical psychoanalysis had been adhered to, he would consider the patient adequately analyzed.

The important point here is that these two analysts were differing on a technical point that was imbedded in a shared network, and as such, was potentially soluble.

Several of these intra-DM debates arose in the symposium, albeit about small points. For example, Loewenstein (1958, pp. 202-203) preferred the term "intervention" to "parameter", as the former was more neutral and thus pointed "more clearly to the need for greater precision and differentiation with respect to these various actions." A little later, (1958, p. 222), Eissler challenged Loewenstein's recommendation of "intervention", stating that interpretations are interventions, they might be confused rather than specifically differentiated and therefore, "in order to avoid further confusion," Eissler suggested that Loewenstein "coin a more neutral term." Loewenstein then rebutted (1958b, p. 241) Eissler with regard to the intervention/interpretation

debates, while explicitly recognizing that there was "some basic agreement between [them]." I would regard the basic agreements between them to be their shared DM. In Kuhnian terms, this minor skirmish would be an intra-DM debate with regard to symbolic generalizations (those consensual terms used across the DM).

In response to gentle pressure from Loewenstein who pointed out that some tools which could not correctly be termed interpretations, nevertheless had the effect of interpretations (Eissler, 1958, p. 224). Eissler proposed the term "pseudo-parameters" (pps). These were used when resistances were sufficiently high to prevent interpretations from being useful (p. 224), and helped the analyst to "smuggle interpretations into the pathognomonic area with a temporary circumvention of resistances." (p. 224) Examples would include the right joke told at the right moment or, the repetition of what the patient has just said (p. 225). According to Eissler, when resistances once again decrease, interpretation can once again come to the fore.

Eissler's pps strike me as a device for acknowledging those actually therapeutic processes that do occur, without according them therapeutic status and without dethroning interpretation as the sole therapeutic tool. The pps concept was an intra-DM accommodation to empirical findings that did not impinge on the paradigm.

Finally, Stone's (1954, p. 572) account of change in classical analysis can illustrate an intra-DM theoretical debate.

We dissolve or minimize resistances, and make the ego aware of its defensive operations, ultimately of id and superego contents and operations. Through this accurate awareness, implemented by the process of "working through", we expect the effect of abolition or reduction of id and superego qualitative distortions and pathological intensities, the resolution or reduction or at least the awareness of intrapsychic conflict in general, and finally the extension of the ego's positive sovereignty over the instinctual life, with the freeing or facilitation of its synthetic, adaptive and other affirmative capacities. In this process, the mobilization of the transference neurosis holds a central place. Whether one views this phenomenon theoretically as essentially a resistance to recall of the past, or an affirmatively necessary therapeutic phenomenon, toward which interpretation and recall are directed for the freeing of the patient from the analyst and thus from internal parental representations, is largely a question of emphasis, which in a pragmatic sense may vary from patient to patient. (emphasis added)

This small theoretical debate about transference exemplifies the potential solubility of theoretical issues if they are embedded within the same DM.

A Kuhnian analysis of these controversies of the late 1940's and the 1950's allows an interpretation that they are in part, ostensible methodological controversies, that is, debates about important DM vs. DD issues expressed in methodological terms, with little awareness that the differences were not primarily at the methods level, but rather at the paradigmatic and DM level--the one fundamental, the other interrelated, and both implicit.

Thus, the relationalists and interpretationists debated in technical terms: about the couch; free association; dealing with acting-out parameters vs. deviations and other modification necessary in treating un-

reliable egos - but rarely addressed their fundamental differences.

A Kuhnian analysis of these debates would lead to an interpretation that these two DMs differed fundamentally with respect to their paradigms and early technical problem solutions; thus, their fundamental differences would include: change mechanisms (working through repetitions in the transference vs emotional re-learning in a reality relationship); locus of change (the ego vs narcissism); techniques (free association and interpretation vs dialogue and relationship; etiology for schizophrenia, unreliable ego vs. lack of self-esteem); conceptualization of disorder (intrapsychic vs interpersonal).

In the debates just documented, these fundamental (inter-DM) differences were expressed in primarily technical terms and the inference is that these are not soluble.

Kuhnian analysis of other controversies is similarly helpful; for instance, another, different group of interpretationists engaged in ostensible debate not with the relationalists, but with the growing trend toward family work in the late 1940's and the early 1950's.

Ostensible methodological controversy between interpretationists and therapists. During this same time, the growing trend toward seeing family members began to draw fire from interpretationist analysts; their criticisms were quite often of the ostensible methodological category. That is, rather than addressing the fundamental differences concerning etiology, operative change processes or mechanisms, and locus

of disorder (individual historical intrapsychic vs. contemporaneous systemic and intrapsychic), the criticisms focused on methodological issues.

Thus, (as reviewed in Chapter III) Edward Glover, Karl Menninger and Leon Saul were all critical of seeing family members of the identified patient. They believed that such contact would be disruptive to the transference or would impede its development altogether; the implication of course is that not seeing the family members facilitates the development of the transference neurosis, which is probably correct. Similarly Kubie thought it unwise for the same analyst to conduct the analyses of both marital partners simultaneously as this practice could well induce one or the other partner to lose confidence in the analyst's impartiality. Grotjahn replied in technique terms; that is, when some degree of paranoid ideation emerged in such arrangements, he recommended either the immediate resort to separate analysts, or planning a "joint family interview" of patients and analyst. He felt that this technical change made the paranoid distortions less destructive; though the technique could not avoid some degree of argumentation, he felt that it did protect the sanity of saner partner by providing some reality testing (Grotjahn, 1960, pp. 68-69, 273 and 281).

At other points, interpretationists criticized the loss of the one-to-one relationship which was regarded as the foundation upon which analysis was built. Its disruption was ascribed to the lack of skill and as obvious attempts to correct the countertransference difficulties

to the analyst. Criticisms progressed from attacks on an analyst's technical expertise and facility to attacks on his/her personality, motivation, or mental health.

It may be hinted darkly that he has an unresolved interest in watching the primal scene. He may be accused of having a "papa complex," or of attempting to become the pater familiae, who God-like guides his flock of sheep. He may be accused of a great unconscious need to play the omnipotent, omniscient, all-powerful, God-like, father-mother. Because of the dependency phobia of our time, he may even be suspected of trying to enslave whole families. (Grotjahn, 1960, pp. 276-277)

Grotjahn's rejoinders regarding the necessity of seeing family members all revolved around the increased efficacy that technique lent to psychoanalytic treatment, especially with intellectualizing patients. Differences in fundamental areas regarding change processes in individuals as family structure were not discussed, rather, seeing family members as a technique was bandied about, as either facilitative of working through emotional material or as destructive of the transference neurosis.

Ostensible methodological controversies involving the DB DM. Some of the controversy which swirled around the DB work could also be characterized as ostensibly methodological. For instance, while referring to the early period of family therapy, Freeman (1964, p. 36) describes seeing the family unit or group as a whole in these terms: "...group problems and group goals are of primary concern and the group process is the pre-dominant methodological frame of reference, with the intent being to exclude the "one-to-one" therapist-individual interventions". (his

emphasis) While no one would quibble with Freeman that seeing families as a whole involved methodological innovations, in light of the DM concept, it's fully as obvious that the "frame of reference" is not merely methodological, but also conceptual.

At times, the controversies have been both very specific and somewhat besides the point. A case in point was an exchange between Jay Haley and Frederic Schlampp regarding some "family experiments" designed and conducted by Haley before dissolution of the DB DM as a functioning group.

Haley and Weakland vs. Schlampp ostensible methodological controversy. Haley's two "family experiments (1962) responded to two separate issues: Haley's interest in the classification of families by characteristic, stable transactional patterns rather than by the diagnostic categorization of an individual family member; and recent criticism the DB hypothesis had met, particularly around the lack of "scientific verification" for its observations and hypotheses. Haley's experiments, by attempting to demonstrate stable and statistically significant differences between schizophrenic families³ and normal families in a laboratory-experimental situation, can be seen as a foray into providing that "scientific verification".

³"Schizophrenic families" in Haley's experiments were families with one so diagnosed child or adolescent member; Haley, it should be noted, was well aware of the difficulties in nomenclature and inter-judge reliability regarding schizophrenia.

Haley's experiments were published in Family Process during 1962; the controversy to be explicated occurred between Jay Haley and Frederic Schlampp regarding the first of Haley's two publications. (As Haley points out, this first experiment had two parts, but as the methodology was almost identical it was published and usually referred to as one large experiment. Its two segments will be reviewed.)⁴ Schlampp also critiqued Weakland and Fry's (1962) paper on "Letters of Mothers of Schizophrenics" but as Schlampp includes their paper in the critiques for the same reasons as Haley's and does not elaborate, Haley's work will be emphasized.

Haley's design. Haley presented a series of assumptions to family study which read like DM tenets:

...(a) family members deal differently with each other than they do with other people, (b) the millions of responses which family members meet over time fall into patterns, (c) these patterns persist within a family for many years and will influence a child's expectations of, and behavior with, other people when he leaves the family, and (d) the child is not a passive recipient of what his parents do with him but an active co-creator of family patterns. (1962, p. 266)

Elaborating on these assumptions, Haley (p. 266) pointed out that the various family groups working with schizophrenia were in general agreement that there were similarities across schizophrenic families

⁴ Haley's experiment will be reviewed only extensively enough to provide explanatory background for Schlampp's ostensible methodological argument and the Haley-Weakland replies.

(other than the inclusion of a schizophrenic member). All these groups had attempted to find some way of describing the "unique kind of interactive process observed when these family members are brought together." Haley's further goal was to "phrase such a description in a way which would ultimately permit quantitative validation of descriptive statements (pp. 266-267). He went on to consider the problem in experimentation posed by working with families, problems of the individual vs. the family as a unit, the family as opposed to the small group, sampling problems of "normal" and "schizophrenic" "families" and the search for experimentally testable hypotheses. A saving grace in all this was the assumption that the formal structure of interacting patterns were stable over time within each family; this was possible because homeostatic factors came into play whenever any behavior or interactional process deviated outside the limits of a family's usual range.

To verify the DB view of families, the incongruence between levels of messages had to somehow be expressed or operationalized. Haley hypothesized that if incongruence were in fact at work, and family members at some level disqualified what each other said, they would have difficulty forming and maintaining coalitions in the family. Clinical observations supported this train of thought, and Haley devised an experimental procedure that allowed family members the opportunity to both form alliances and to communicate at two levels. (Haley, 1962, p. 281).

Briefly, Haley chose to work, for simplicity, with a three-person system of father-mother-formerly schizophrenic child.

Father, mother and child are placed at a round table with high partitions so they cannot see each other. In front of each person there is a small box with a window in it. This is a counter, like an automobile speedometer, which runs up a score visible only to the person in that area. Also in front of each person there are two buttons which are labeled for the persons on the left and right. That is, in mother's position she has a button labeled 'husband' and a button labeled 'son' (or 'daughter' as the case may be). Besides these two buttons, which we shall call the coalition buttons, there are two more buttons, one on each side. These are signal buttons. When pushed, the signal button lights up a small light in the area of the person on the other side of the partition. By pushing either of these two buttons, for example, mother can signal father or child. All of these buttons are connected with pens on an event recorder in the control room so that all button activity is recorded during the experiment.

The table is wired so that the counters begin to add up a score whenever two people choose each other by pressing each other's coalition button. When mother presses the button labeled 'husband,' nothing happens until father presses his button labeled for her. When both buttons are pressed at once, then both counters add up a score at the same speed and continue to do so (making an audible sound) as long as both buttons are pressed. Therefore each person can gain a score only if he joins another person, and then he and that person gain exactly the same amount of score. Each person can signal another with the signal button to invite a coalition. The family is asked not to talk together during the experiment so they can only communicate by button pushing.

Father, mother and child are placed at this table and told this is a game they are to play together. They are instructed that they should each try to win by getting the highest score. They may push buttons one at a time or two at a time or not at all. The only rule is the prohibition against talking during the game.

The 'game' consists of three rounds of two minutes each which are begun by the experimenter and ended by him. At the end of each round the family members are asked to read off their

scores, and then the next round is begun without setting the counters back to zero so the score is cumulative... In addition to the three round game, the family is asked to have a fourth round. Before they begin the fourth round, they are asked to talk together and decide who is to win that round and who is to lose. Then they are to have another two minute round and see if they can make the scores come out the way they planned. This conversation is recorded. (Haley, 1962, p. 284)

The design, according to Haley, would reflect the family member's difficulty in forming and maintaining stable coalition in the family, as an expression of their disturbed family structure and communication.

Haley's sample consisted of sixty families; the thirty normal families were selected by random choice from students in a high school directory. The parents were telephoned and the sample was comprised of those families whose members had not had psychotherapy and who would be willing to come in to their laboratory for the experiment. The children ranged in age from 14-17, with thirteen girls and seventeen boys in the sample.

The thirty schizophrenic families were chosen by availability from a family therapy program, the records of state hospitals and other included children actually hospitalized at the time. The children ranged in age from 11 to 20, with only three girls and twenty-seven boys. Haley adds that a more equal distribution could not be found. The educational levels were slightly lower in this group.

Of the sixty families, twenty normal and twenty schizophrenic were instructed that they could push buttons in any way they pleased and therefore they could form coalitions with either one or two people simultaneously. This was the first experimental condition. In the

second, the remaining ten normal and ten schizophrenic families were instructed that they could form only one coalition at a time and therefore could not score with two people at once. (Haley, 1962, p. 285)

Schlamp's methodological critique. Schlamp's (1964) critique is interesting for a number of reasons. First, several of his comments were of methodological interest; these will be pointed out as they are encountered. His appreciation of the two papers ("Letters" by Weakland and Fry and Haley's first "Family Experiment" paper) is evident, and he points out that among those in family therapy, only the DB group had produced "controlled experimentation." (p. 229) He is generally very approving and focuses immediately on their research methodology, citing the two papers for having presented "meaningful, systematic and testable hypotheses within an experimental frame of reference." (p. 229) At no point in his analysis does Schlamp step beyond the consideration of methodology and at no point does he take the DB system as a system into account, preferring to critique the system via methodological criticism, especially of Haley's paper. My impression is that Haley's paper is particularly singled out as it is couched in Schlamp's view of legitimate science. It is an experiment, in a laboratory no less, and as such approaches a form of discourse resembling Schlamp's. Thus, he would be on familiar territory and also more likely to attend to this particular form of demonstration.

Schlamp then presents his description of a normal family, then

some alternative interpretations of Haley's data, specific methodological criticisms, and, finally, some suggestions for "more critical research designs."

Because Schlamp's description and characterization of a "normal family" is absolutely central to his critique, it will be reviewed as he presented it.

Let me first describe what I will call a 'normal' family. Let me further hypothesize that one son from this 'normal' family has been hospitalized with a diagnosis of schizophrenia. The parents and the siblings, but particularly the mother, is quite concerned for her son. The concept of mental illness is not out of the range of experience for this family. The dread, fear, awareness of social stigmata, and the feelings of personal inadequacy are felt by them in the same way that it is felt by many families of hospitalized psychotic individuals. Over the period of years the parents have built up a deep love for their son and are very concerned about the likelihood of his recovery. Both of the parents, and to a lesser extent the siblings, of the psychotic patient wish to do all they can to help him recover and to help him share in an active outgoing life. They are unaware, however, of the nature of psychosis. They have many quasi superstitious attitudes and unspoken feelings concerning psychotics, and are quite honestly ambivalent in many areas about what they 'should' do. The son, on the other hand, although in remission, shares the customary aftermath of a psychotic episode in that he is somewhat apathetic, has problems with communications, probably centering around his own ambivalent feelings, and yet once having committed himself to a method of action, he can maintain this adequately for some time before his own self doubts and recriminations (partially unconscious) block further single purpose behavior.

In such a situation we introduce the family experiments of Haley. Essentially this consists of father, mother, and the schizophrenic child forming various 'coalitions' with each other. This means that a button is pressed signalling a desire for a coalition with another partner and leaving the way open for a mutual scoring between these two partners. Implied in the instructions to the 'players' are that each person is supposed to try to 'win', probably with some

reference to seeing how they work as a family, or at least there will be implied to most parents "how good are you?" In this situation the parents I have described would be most solicitous towards their "sick" son, and yet also anxious to do well. The son, on the other hand, would be slow to respond both in pressing coalition buttons and in releasing coalition buttons. That is, once having responded, he would tend to persevere for a period of time, as is characteristic of many schizophrenics in partial remission. (Schlamp, 1964, pp. 229-230)

Schlamp then specifies that he will examine the hypotheses and conclusions of the Haley article, "under these circumstances," i.e., under the circumstances he has just added, which constitute in effect a rudimentary DM of his own and different from the DB DM. Schlamp has introduced several assumptions that the DB DM does not; these include: the mother being particularly concerned, the operative presence of deep love developed over the years, the efforts to help the identified patient recover, the uncertainty about the best course to take, the quasi-superstition, the son "in remission" with this "customary aftermath" of apathetic behavior, including problems in communication about ambivalent feelings and finally the capacity to function in a concerted course of action once committed. While the DB DM might well "allow" some of these assumptions (e.g., mother's particular concern), they are irrelevant to the DB formulation. Moreover, many of them partake of "internal processes" which the DB DM explicitly eschewed.

Essentially, Schlamp's description establishes for him a DM and allows him to view Haley's data through his own (Schlamp's) DM, which makes possible the alternative interpretations he makes of the data.

By arguing over these alternative interpretations, time and energy is spent on what appears to be a process of elucidation whereas it more realistically resembles a man shooting an arrow at the image seen from crossed eyes. By examining Haley's methodology and data through his own DM, it is not surprising that Schlamp comes up with different interpretations, conclusions and methodological demands.

He initially considers Haley's first hypothesis, that schizophrenic families would have more difficulty forming and maintaining coalitions (and would therefore have a higher percent of time when no member of the family was in coalition with any other member). Haley reports this hypothesis to be supported at the .05 level (1962, p. 286). Schlamp counters with:

It is not surprising that a significant difference at the .05 level was found. All that would be required would be a slower and more apathetic pressing of buttons by the schizophrenic member. Simple statistics will show that in this case the difficulty of maintaining any continuous coalition is reduced by two-thirds. (Since of the three coalitions possible, mother-son, mother-father, father-son, two of these are reduced or eliminated.) Under such circumstances one would expect to find that the difference was significant only at the .05 level, under such circumstances. (1962, p. 230)

"Under such circumstances" anything might be possible. The point is that Haley was not operating "under these circumstances." His DM did not include the presence of an apathetic post-psychotic button-pusher, and thus Haley's interpretation and significant .05 finding stand legitimate, notwithstanding Schlamp's re-interpretation and statistical critique.

Methodologically, Schlamp's introduction of apathetic behavior is even more dubious. He uses the term in his analysis in a way analogous to what economists call a "shock variable." Shock variables are those variables introduced to explain why a prediction or analysis did not function as anticipated. Thus, the soybean projection would have been accurate if it had not been for the 1977 drought, the drought functioning as the shock variable; or, the GNP would have achieved such and such levels had it not been for an unexplained coal strike (oil embargo, unexpected government policy, etc.). Schlamp uses apathy in this way. It allows his analysis to answer empirical events, even though the shock variable has no integral role in the rest of his analysis, and certainly not any role in Haley's DB DM. "Apathy" bears no relation to Schlamp's other assumptions and in fact, is tacked on to the end of his description, with no connections to the rest of the analysis. Yet it allows him to interpret, into his own DM, Haley's first finding and then several subsequent to it.

So, he again uses apathy, to re-interpret Haley's second hypothesis that the family of the schizophrenic would have longer continuous periods of time when no two family members were in coalition. (This hypothesis was supported in Haley's study; Haley's interpretation, in DM terms, is that this demonstrated the difficulty schizophrenic families had in forming and maintaining coalitions compared to "normal" families. Though Schlamp does not in this case dispute the method, or the empirical finding, he re-interprets the finding so that it "fits" into his own DM; he

does this by using the shock variable of apathy and one other of his assumptions, parental solicitude that translates into solicitous behavior. Thus:

Here the same criticism noted above is important. The apathetic behavior of the schizophrenic member could easily account for the difference. In addition to this, however, we must remember that the parents are solicitous of their schizophrenic son. As a result, they do not wish to pile up too high a score and thus either make him feel uncomfortable, show him up, or make it appear to the experimenter that they are taking it out on the son. (Schlamp, p. 230)

Schlamp then uses two other of Haley's findings to support his own interpretation. By re-interpreting the finding in the first place, he supports his own DM using Haley's empirical results essentially out of context, i.e., "imported" into Schlamp's DM. As such, they supposedly lend strength to his own view; in actuality, of course, they do no such thing. Though both men are talking about coalition behavior, the meaning of each instance of this behavior is very different, and the implications for the remainder of each of the two respective DMs are similarly different. This is because sub-sets of concepts and empirical findings are of course, not necessarily the same across DMs and thus "coalition behavior" has a different "place" in each DM, with a different set of articulations to other concepts, different meanings and sources of verification.

By supporting his interpretation through the use of two of Haley's findings, Schlamp compounds both the spurious support of his own interpretation and the illegitimate form of methodological critique. After

receiving this critique, Haley felt compelled to rebut it. Such a rebuttal could have taken the form of addressing the methodological critique (as Haley did), addressing "errors" in the critiquer's approach, citing findings supporting one's own work, or embarking on a search to find the "crucial experiment" which will convince both sides that one's approach and interpretation are, in fact, correct. What all of these partake of structurally is ostensible methodological debate. And as the debate becomes more and more elaborated with critique, counter-critique, and supporting data taken out of context, it becomes easier to focus on one tiny methodological point of difference and invest it with terrific importance, as implicitly it carries the burden of an entire DM behind it, and expresses the multitude of differences between two DMs in conflict.

This is what occurred with the Schlamp methodological critique. At no point does he deal with their DM differences (in any vocabulary or system of thought), but rather he presents his methodological critique as though he were within the same system as Haley. Any indication of their basic differences must be inferred by reading between the lines, as Haley sometimes does.

For instance, at several points Schlamp reinterprets findings in such a way that the explanatory concepts are placed within the schizophrenic child. Haley's second hypothesis predicted "the family with a schizophrenic [member] would have longer continuous periods of time when no two family members were in coalition." (Haley, p. 286) Schlamp

addresses the second half of the experiment, where individuals could score with only one other person at a time and not both. The difference was significant at the .01 level in support of the hypothesis that the schizophrenic families would have longer continuous periods of no coalition. Schlamp challenges Haley's method, then his interpretation, subsequently forwarding one of his own.

...the difference was significant at the .01 level on this hypothesis. This may mean logically that the differences between families is greater in the latter experiment. But what would cause this greater difference? If the parents really had difficulty communicating with each other and with the son, as well as the admitted difficulty of the son in communicating with the parents, the experimental restriction of being allowed to score with one person only would not have much effect upon this family since they are having difficulty scoring anyway. That is, since both coalition buttons may be pressed by any player without penalty the restriction of 'only score with one at a time' would have less effect upon a group of players that already was having a difficult time scoring. This is true because the restriction does not fine the two who would have been scoring together anyway. With families who have no difficulty in mutual scoring, their responses will be sensitively and mutually regulated between one and another.

The normal family thus should be more seriously impaired by this restriction since they are communicating more sensitively and accurately to determine their own and other member's score. Thus the differences, when tested for this particular hypothesis, should be less in the second experiment rather than greater. This can be resolved by imagining, as we have done here, that the difficulty lies not with the family, but with the schizophrenic son. (Schlamp, p. 231; emphasis added)

If Haley's method is examined in the light of Schlamp's DM, the latter's methodological critique is well taken and in fact, the difference should indeed be less in the second experiment rather than greater. However, that is not the point. The point is that in the research

critiqued, the differences were greater in the second experiment as they "should" have been in Haley's system. Schlamp is attempting to explain away a predicted and demonstrated finding within Haley's system because it does not agree with Schlamp's own interpretation of what should happen. This occurs in the process of what is titled a methodological critique. Schlamp has taken his own DM as a starting point here and has built a method and interpretation around it, faulting Haley's for not being the same! In these situations, a Kuhnian analysis would imply that because of the strong and usually implicit commitment to one's DM, it is very difficult for participants to notice that their methodological disagreements are embedded in DMs based upon different paradigms, of which they fundamentally disagree.

At issue here are two DMs, and particularly the differences between them that relate to the issue of individually vs. family based psychopathology. Schlamp specifies that the methodological difficulty about hypotheses "can be resolved by imagining, as we have done here, that the difficulty lies not with the family, but with the schizophrenic son." (p. 231) Haley picks up on the conceptual discrepancy between the individual and family approaches somewhat, but also attempts to counter Schlamp methodologically.

Conceptually, Haley does address the individual vs. family approach. He addresses the necessary conceptual shift a "family caused" process requires and the "century of investment in the idea that psychosis is produced and persists independent of the current life experience of the

patient." (p. 240) He also points to the temptation of trying to find a crucial experiment to demonstrate once and for all whether psychosis "has a 'cause' within the individual or whether it is a product of a network of relationships." (p. 240) He further explicitly refers to the shift in viewpoint necessary to seek the cause: in the "context of relationships rather than within the individual." (p. 241)

Haley's comments about Schlamp's methodological recommendations at points highlight the difference between the individualistic vs. family orientation. Thus, he is able at times to counter Schlamp's methodology suggestions with conceptual points. For instance, Haley objects to Schlamp's recommendation about having non-schizophrenic parents participate with schizophrenic boys on the basis that his task was "not to measure the differences which occur when an individual from one family is placed in circuit with another family, it [was] to measure his habitual patterns in his own family." (Haley, p. 243). The tenor of Schlamp's recommendation indicates that he was an individual theorist and did not really understand the underpinings of the family approach. Moving a disordered child in to a "normal" family would not suggest itself to family therapists as a way of finding out about the disordered family. Actually, it probably did not occur to Schlamp either; his focus was on the child rather than the family.

As mentioned previously, Haley at times responded conceptually, giving some sense of resolution to an issue; in the preceding example, by pointing out that the methodological recommendation was inappropriate

to an individual-focus approach but not a family-focus, the recommendation is acted upon in such a way that a decision has been reached. The recommendation is useful.

At other times, Haley met Schlamp's methodological critiques with methodological defense. With regard to another aspect of the same issue (the individual vs. family focus) Schlamp criticizes Haley's research using a chicken-or-egg argument.

There are obvious changes in the experimental design which will test the alternative hypotheses. This will be touched upon in the following section. One of the difficulties in attributing the schizophrenic process to either the double bind frame of reference, or to some other difficulty in communication within the schizophrenic family is that, particularly with a psychotic or postpsychotic individual included in the family sphere, one is never certain that the breakdown of communication is due to the presence of the psychosis, or whether the psychosis has caused the breakdown in communication. Exactly the same dilemma was presented in an earlier article by Weakland and Fry (1962) "Letters of Mothers of Schizophrenics." (Schlamp, p. 233)

While Schlamp's basic question is of course valid, his use of it here as a methodological critique is not. Haley's experiment was designed to measure and quantify certain family interaction, not to assign causality. To answer Schlamp's question, the researcher would need a long-term prospective study -- an enormous effort in time, energy and money entirely appropriate to the etiological issue, methodologically powerful, and directed to criticisms leveled at the DB DM as a whole. Schlamp's critique here is demanding that Haley's experiment meet criticism applicable to the DM as a whole. If Schlamp had directed this point to "the DB hypothesis" or any such rubric, his point would

have been valid. Couching an inter-DM issue in methodological terms is not valid and saddles the experimental method with more meaning than it has discretion over. Unfortunately, Haley engaged Schlamp's critiques here on a methodological rather than conceptual level. At one point, for instance, Haley (1962, p. 240) indicated that once the family point of view is pursued, the next response is to look for a "crucial" experiment which will conclusively demonstrate whether psychosis has a "cause" within the individual or whether it is a result of a network of relationships.

There lies the rub. It is conceivable that either of two kinds of investigation might answer the question: (1) a demonstration that there is some chemical or organic difference, not environmentally caused, between the psychotic and the average individual. In the last half century an overwhelming amount of research has failed in the endeavor to demonstrate this. (2) Rigorous experiments could be devised to show that the interaction between parents and psychotic child is different from that between average parents and child, thereby supporting the hypothesis that the child is psychotic because of a particular sort of relationship with his parents. In this endeavor we face a difficulty which is becoming more evident. If the psychotic child is included in the test interacting with his parents, it can always be argued that he "caused" whatever results are obtained, yet the child must be included if we are to test the way he deals with his parents and they with him. (Haley, p. 240)

In the DB DM, the schizophrenic "child" necessarily had to be included in the interaction, yet that left any research design, but the long-term prospective project, open to Schlamp's criticism. Unfortunately instead of pointing out that the critique was an inappropriate demand for research already completed,⁵ and that it criticized a point he had, in

⁵Haley (p. 240) indicates that his experiment was a "first attempt to provide families with a similar context to see if measurable differences could be found between supposedly different types of families."

in fact, not made, Haley joined in methodological debate with Schlamp over this point (pp. 240-243). The heart of Haley's retort is that the argument that the schizophrenic child "causes" the difference between types of families in such experiments is irrefutable (p. 242) in the sense that it cannot be proven nor disproven and is essentially a universal explanation (p. 43). Haley pointed out that essentially whatever findings were obtained in the experiment could be explained by the disturbed child interpretation.

If the members of the schizophrenic family have fewer and shorter coalitions, it is because the abnormal child is unresponsive. Should the opposite have occurred and they had shown more and longer coalitions, it could be argued that the distress of the child brought them together more. If the parents are more in coalition than normal parents, it is because their relationship is "normal." If they should have gotten together less, it would be because they are busy trying to gain a response from an unresponsive child. If the father wins the game, it is because the child is such a problem that the family is under stress and mother must lean upon father. If the father should have lost most of the games, it could be said that mother was too preoccupied with her difficult child to get together with father. If the family coalition time is less than normal, it is because the child is erratic and unresponsive. Should the time be longer than normal, this is caused by the child perseverating. When the mother presses her buttons frequently it is because she is both "normal" and doing her best, and if she should be unresponsive and not press her buttons it would be because of apathy resulting from having a disturbed child. (Haley, p. 242)

Obviously, Haley was nettled by Schlamp's re-interpretation of findings. However, instead of pointing out that the re-interpretations were irrelevant to his point of view, or that they constitute a perceptual and conceptual system of their own, Haley engaged in a methodolo-

gical reply that left the two conversants at an impasse. While Haley's point regarding the irrefutability of the disturbed child interpretation is "true", it is undoubtedly not the sort of argument to daunt Schlamp, (particularly as the disturbed-child idea is central to Schlamp's position. He cannot "afford to abandon it"). This leaves Haley's rather nice point not convincing Schlamp, Schlamp's interpretation not convincing Haley, and the methodological argument unresolved and available for yet another round of ostensible methodological debate.

Lest Schlamp's other methodological criticisms be overlooked, it should be noted that he further criticizes Haley's overlooking of a significant chi-square for hypothesis 6 (p. 233), proposes four designs necessary in follow-up studies (pp. 234-235), for Weakland and Fry's paper (1962) as well as three "critical experiments" for Haley's work (p. 235) and "a more appropriate method" based on three-person non-zero-sum non-negotiable games (p. 235). He ends with the common left-handed compliment regarding the experiment's usefulness in generating new hypotheses and stimulating further research. (p. 236) Within the context of his own DM, it should be emphasized, his points are well-taken and potentially helpful. Directed across Dm boundaries, they are irrelevant and obfuscatory.

Haley responds methodologically more often than not. For instance:

A reply to these alternative explanations could only be made with data on individual families which were not included in the article. To some extent the total figures do not reflect the great variation found in the schizophrenic families, and although the schizophrenic child

pressed his buttons significantly less often, he really wasn't as unresponsive as Dr. Schlamp implies and as he would need to be for Schlamp's explanation of the results. (p. 241)

Haley⁶ further criticizes Schlamp's alternative experiments (p. 243), and then proposes an experiment that methodologically controls for the disturbed child's influence, but which would not give him a measure of parental response to their child, "which is presumably the goal of this type of research." (p. 244; emphasis added). Thus, to meet Schlamp's central methodological criticism, Haley must give up the central goal of his research! That, it appears to me is the hallmark of an ostensible methodological controversy.

Inter-DM symbolic generalization differences. When two DMs differ as drastically as those represented by Schlamp and Haley, with differences in crucial areas (e.g., individual vs. family interpretations of data), it is relatively easy to see why ostensible methodological controversy can lead to long-lasting sterile controversy. Yet at times,

⁶Weakland's reply appears fairly evenly split between pointing out their (Weakland/Fry and Schlamp) conceptual differences and engaging in methodological debate. Thus, he comments on conceptual differences regarding individual or family approaches (pp. 236-237 and p. 237), addresses the disturbed child alternative interpretation (p. 237) and later objects to it as an "alternative line of explanation" (p. 238) and finally comments that there is no crucial test yet and there is not likely to be one (p. 239). Methodologically, Weakland quibbles about Schlamp's definition of normal family (pp. 235 and 237) and defends the letters as "interactional" against what he terms Schlamp's "scientific objection" (p. 239). On the whole, however, Weakland managed to avoid a good deal of the ostensible methodological debate.

even less crucial inter-DM differences add their weight to the confusion. A case in point is an interchange between Jackson (1963) and Paul over symbolic generalization.⁷

In 1963, in a comment in Family Process, Jackson broached the subject of language, implying that when family therapy gained a better theoretical base, the necessity for a new language would arise; in particular, differentiation from the language of psychoanalysis would be necessary (p. 182). Interestingly enough, Jackson comments that the "individual-oriented framework" of psychoanalysis and the "group interactional" framework of family therapy are essentially "discontinuous systems" and that the language of one will not be appropriate for the other. Moreover, the two systems are not strictly comparable and therefore, one cannot be better than the other (p. 182). (These are remarkably Kuhnian views to be appearing in 1963. While Kuhn's first edition was published in 1962, it seems unlikely that Jackson could have so quickly encountered and integrated it into something published early in 1963. In any case, Jackson's view here is quite like Kuhn's 1970 revision).

Jackson felt that psychoanalytic terminology was often too imprecise to risk further imprecision or confusion by its being "dragged into

⁷"Symbolic generalization" was the term used by Kuhn for a DM component. "Symbolic generalization" denoted those terms or expressions used consensually within a DM; thus, within a DM, such a term referred to the same thing for everyone, and it conveyed the same meaning. The same term used in a different DM might refer to something else, or a different aspect of the same thing, or mean something else.

another field of usage" (p. 182); he demonstrated his point by discussing some of the confusion engendered when terms were applied to families (pp. 182-184), mentioned such terms as "transference and countertransference, sadism and masochism, ego strength and Oedipus complex." (p. 184) Finally, he recommended that the temptation to borrow a language ready-made be resisted, and that family therapists begin constructing their own language system.

In the next issue of Family Process, Paul challenged the necessity of a new language, pointing out that "there is a language available in which human conduct, passions, relationships and dealings with one another can be described. This is the everyday language." (Paul, 1963, p. 397) This was also an example of an obvious and irrelevant point. Jackson was arguing against the use of psychoanalytic-terminology (and by inference, language from other psychiatric thought systems); he was not recommending neologisms.

Paul's recommendations and illustrations run afoul of just the problem Jackson was addressing. Among Paul's recommendations is the term "role." Jackson points out that the usual individualistic orientation of this term, its connotation from other systems and the difficulty of using such a term for interactional processes. He finishes by commenting that "The usual definition of role is quite different from the one I have offered." (Jackson, 1963, p. 397)

While not strictly a methodological controversy, this small debate is valuable in highlighting what happens when one person in a debate

realizes that there is a discrepancy in symbolic generalizations. Jackson's original point about borrowed terminology appeared clear enough. A retort followed that apparently misread Jackson's meaning, or if not, that was patently irrelevant; recommendations were made and Jackson commented on the symbolic generalization discrepancy. ("The usual definition of role is quite different from the one I have offered." (Jackson, 1963, p. 397) As such, he averted a possible sterile controversy over a DM component. Such a controversy, for instance, could take the form of two people arguing that their respective term was the appropriate one for such and such ("such and such" of course being not identical across the DM boundary and therefore though both are "correct" for their own meaning of "such and such," neither will prevail and convince the other to adopt their usage, as that usage will be "incorrect" in the other DM).

When DM differences are dealt with on a DM level, however, the controversies are resolvable.

A Kuhnian analysis, of certain methodological controversies, then, has allowed the interpretation that what differed between each of the pairs of groups arguing was paradigms and DMs; method or technique also differed, but were not the sole area of difference, nor necessarily the only such area. A Kuhnian analysis allows us to infer a number of things. First, the debates were between Dm, based on different paradigms. Second, method or technique was a focus of debate as it was a relatively explicit component of what we have termed the DM; thus, it at least was

available for debate. Third, inter-DM technique or methodological debates will not be resolved; this type of debate is an expression of more fundamental differences (paradigm and DM) which would remain even if the technical debate were resolved, which it necessarily could not be, as technique and method are not independent of either paradigm or DM.

Differences in technique based upon different paradigms cannot productively be debated as though they were independent of their respective paradigms. For these reasons, the debates about technique between the relationalists and interpretationists, and the debate about method between Haley and Schlamp were unresolvable at the technical and methodological level.

The fourth possible inference is that this is the case for some other methodological and technical debates; some of these, if analyzed with the present modified Kuhnian schema, will be seen to be inter-DM differences being argued at the wrong level. Fifth, inter-DM differences debated at the DM level itself will probably have greater potential for resolution. While the attitudinal and commitment aspects of DMs would of course, make agreement or conversion unlikely, these aspects would not necessarily preclude resolution. A successful such resolution might be the identification of points of difference, differentiation of trivial from crucial differences, and the avoidance of protracted sterile ostensible methodological controversies. Because these controversies are an expression of often fundamental differences, they are often invested with acrimony and frustration; as they are essentially unresolvable, they continue and continue, draining effort from more fruitful endeavors and

further obscuring the issues.

A sixth inference would be that controversies likely to fall into the "ostensible" category, and hence to benefit by this type of interpretation, would be controversies that: were long-standing and protracted; appeared acrimonious at times; took place between what appeared to be adherents to some sort of rival approaches or "schools"; and had produced no resolution or progress toward resolution. More subjectively, these debates may have a somewhat "ostensible" feel to them, as though the participants were going over old ground and they were well-rehearsed.

With the present modifications of Kuhn's schema, these types of controversies can now be interpreted such that resolution is at least possible.

Group Structure of Scientific Activity

Use of Kuhn's schema to examine the emergence of family therapy emphasizes the group structure of research and practice. Particularly because of Kuhn's DM concept, with its procedural, theoretical and conceptual consensus across a group, the analysis allows several elements to emerge.

Facilitation of consensus, cooperation and productivity. First, as has been mentioned with respect to the DB, commitment to the same DM allows several individuals to work together, despite some level of individual differences. Obviously, the differences, if too great, preclude adherence to the same DM, or contribute to its splitting; nevertheless,

within certain limits, a DM serves a cohesive and facilitating function for a group of individuals and as such, engenders relatively high rates of "productivity." An interesting question would be to inquire what constitutes, specifically, these limits. Also, presupposing DMs to differ in their ability to "tolerate" individual differences, what characteristics would make for greater and lesser "tolerance?"

A group of people, working in concert, is often more productive than those identical people working in isolation; the existence of several individuals working in concert within a shared framework was very important to the DB's development and influence.

Interrelated commitments of the group practice. The DM concept also emphasizes how interrelated its various components are, and thus, indicates how interrelated the commitments are among practitioners that share a paradigm. To take an illustration from the DB DM, it becomes clear that one reconceptualization necessarily leads to another, and another; thus, the concept of homeostasis led to the extension from "victim" and "binder" to three-party systems, then family rules; moreover, reconceptualizations became operationalized through the DM's particular set of methods and techniques. (Conversely, it will be pointed out later, different techniques and methods between DMs are often expressions of different sets of conceptualizations.)

Thus, for the DB DM, one change in thinking inevitably led to others.

When one accepts the idea that a problem involves more than one person and is a response to a current situation,

it necessarily follows that symptomatic behavior is appropriate behavior. The system has an adaptive function in the person's intimate relationship and is not irrational or maladaptive. The diagnostic question is what sort of situation is provoking this kind of adaptation. For example, if a person is depressed, the question for the family therapist is not what type of person is this or what past experiences have led to this behavior. It is what function does the depression have in the current situation and how is it appropriate to what is happening...The logic of the family view leads to a reversal of traditional thinking about the cause of behavior. Rather than assume that a person has a predisposition to collapse into abnormality under stress, it is assumed that each person's stressful situation is different. Similarly, rather than assume that a person seeks out certain kinds of relationships because of his inner dynamics, it is assumed that his dynamics are a result of the relationships in which he lives. What was considered primary and secondary gain is reversed. (Haley, 1971b, p. 282)

The internal coherence of a DM points to the internally consistent set of commitments which underlie the activities of scientists working on the same (or a very similar) set of problems from the same perspective, and as such, underline the often implicit group structure of such activity. As such, recognition of the group structure of science and the internal coherence of DMs can help to avoid a prevalent situation in what Masterman has called the social and information sciences. Masterman talked about the lack of long-term progress in these areas as the result of multiple paradigms. In a multiple paradigm area, Masterman points out that technology develops to high levels, with little in the way of guiding framework or goals. If the interrelatedness of DM commitments is kept in mind the willy-nilly extension of methodology and high technology from a DM to some problem outside it

will seem less valid and legitimate, unless the DM's other components are also tested on the problem. Then, the method and its context are not treated as though they are independent, which in fact they are not. At the present time, when the two are treated as though they were independent, we end up with a sort of "mercenary methodology."

Priority of procedure over theory. Similarly, the present Kuhnian analysis has emphasized the tacit⁸ or procedural aspects of scientific activity. Usually, the explicit aspects, whether theoretical or methodological receive the lion's share of attention, and are in some ways better understood. Hence, Anna Freud's (see Chapter IV, p. 58) argument that technique follows theory.

A Kuhnian analysis by emphasizing the implicit procedural elements, particularly with respect to the problem-solving "trick", which becomes a paradigm, obviously turns this priority on its head. Technique often develops prior to theory; at least, the "early technical problem-solutions" that in part comprise paradigms are prior to the theory. The theory usually emerges during the elaboration of the paradigm. Then obviously, more techniques and procedural elements follow as part of the DM.

The application of the Kuhnian analysis to the emergence of the DB DM demonstrates the priority of procedure. So, in 1971b(p. 4), Haley

⁸"Tacit", obviously, being Michael Polanyi's well-known term for the non-explicit aspects of what a person knows or what s/he can do.

stated:

Actually observing families and trying to change them produced information which had never been gathered before. Rather than family therapy's developing because of a theory, it appeared that people were struggling to find a theory to fit their practices. There was no theoretical model which could be used to describe behavior in natural, ongoing groups, and there was no language for describing their relationships.

Similarly, when Beels and Ferber (1972) watched family therapy through one-way screens, examined video-tapes and interviewed family therapists, they decided that "We avoided the evaluation of theory because we believed that in many cases the theory advanced was a rationalization for the practice..."

Also, once a paradigm has been developed, the DM is often extended procedurally, with the theory following.

It would be preposterous to claim that from the outset our work was oriented along such advanced epistemological principles. Rather, what led us in this direction, for which we may invoke epistemological justification a posteriori, were eminently practical, mostly clinical considerations which we were able to conceptualize only after Gregory Bateson and his original research team at the Veterans Administration Hospital in Menlo Park had begun to apply anthropological and cybernetic rather than psychiatric principles to the study of families with an emotionally disturbed member. (Watzlawick and Weakland, 1977, p. 1)

During DM development, or when an individual attempts to "enter" or "learn" a DM, these tacit, procedural elements may be very difficult to articulate and may require a good deal of trial and error by the "outsider" attempting to join the group structure.

Attempts at formal training in family therapy also began

in the 1960's. This brought questions, such as "What ideas about the family are relevant to therapy?" and "How can an experienced family therapist teach what he knows how to do but has difficulty in describing?" When it is recognized that family therapy is not a method of treatment but a new orientation to the human dilemma, it is clear that any number of methods might be taught and used. With experience, family therapists often shift from a method approach and become more problem oriented, adapting what they do to the problem that has come in the door. Since students like to have a "method" which they can learn, family therapy is difficult for them to grasp. They must absorb a new orientation which is different from the one taught them in school, and they must learn a problem approach which can only be learned from experience with different problems. (Haley, 1971b, pp. 6-7; emphasis added)

Essentially, rather than being taught the approach, Haley is advising learning by "ostension", Kuhn's version of learning by relevant and repetitive problem exposure. Rather than explicit teaching and learning, the process resembles socialization over time, similar to what is found in professional or graduate training programs.

Similarly, the tacit elements often include values, a DM component recognized as influential in scientific activity, but usually disregarded, at least, by other meta-scientific systems. Values operate across a scientific group, influencing problem choice, approach and standards of solution. These values in fact, operate as one of the few "automatic" points of agreement; that is, they are usually not subjected to verification or debate, in part because they are seldom recognized.

"DM" rather than "discipline" helpful. The emphasis in group structure is helpful in two more respects on different levels. First, by regarding scientific activity as usually occurring in a group structure, formerly

chaotic situations gain comprehensibility. For example, Haley's account of the multiple of therapeutic innovations in the 1950's lends itself to reinterpretation along DM or group structure lines.

The range of possible therapeutic innovations opened up along with a range of possible therapeutic approaches. In the 1950's a number of therapists began to bring whole families into treatment sessions with a goal of changing the relationship among the family members. Often a therapist began to do this without knowing that others were doing it too. Within a few years, about ten or twelve distinct schools of family therapy had developed and different approaches continue to appear today. These different family therapists do not necessarily share a common method, but they share the idea that the unit with the problem is more than one person and many of them have shifted to a unit of a triad or larger. (Haley, 1971b, p. 277)

Finally, as Weimer and Palermo (1973, p. 239) have pointed out,

Kuhn has made this point well in 'Reflections', discussing the problems of characterizing a particular incident as normal or revolutionary. To decide in the particular case one must specify normal or revolutionary for whom. The unit of analysis then becomes the research group rather than a scientific subject matter or institutionalized academic discipline.

This shift in focus, from "scientific subject matter" or "institutionalized academic discipline" to a particular research group proves helpful when perusing the scientific landscape. For example, early in this essay (see Introduction), I found it intuitively necessary to choose a "school" of family-therapy, the DB group, as my focus, rather than all of family therapy; moreover, a disclaimer was necessary, to the effect that "family therapy" as an activity spanned a number of academic disciplines and professional groups (i.e., psychology, sociology, education and Ph.D.'s, M.D.'s, M.S.W.'s, R.N.'s, etc.). Attempting to delineate

"family therapy" by using the usual recognized categories did not work, whereas focussing on one of the groups which comprised family therapy did work. The family therapy field is more helpfully categorized or viewed by DM rather than by academic or professional discipline; family therapy is comprised of several distinguishable DMs, none of whom respect disciplinary boundaries, sociologically or conceptually.

This view is helpful in other areas of the social sciences as well; for instance: psychosomatics, forensic psychology and psychiatry, neuropsychology. While it is quite possible that any of the above can be investigated or scientific activity can proceed within one discipline (e.g., medicine, psychology, psychiatry, psychoanalysis, stress research for psychosomatics, etc.) the relevant structure is usually not the name of the discipline (or even sub-discipline) but rather the problem addressed and the group working on it. In scientific practice, these groups are often interdisciplinary and individuals identify themselves with the problem/working group rather than the disciplines at large.

To a large extent, the ability of the Kuhnian schema to highlight the group structure of science was made possible only after Kuhn began to explicate the DM idea. While the paradigm concept implied a group structure, the relationship was sufficiently ambiguous and tenuous that no one wrote of its possibilities in terms of group structure. With the DM obviously a group concept, the group activities and processes in scientific activity emerged. Using the DM concept as the relevant structure of scientific activity, a Kuhnian analysis allows

interpretation of the relationship of the interpretationists, relationalists, and family therapists than an analysis in academic terms would preclude.

Relationship of DB family therapy to classical psychoanalysis. The Kuhnian analysis allows us to interpret the relationship between DB family therapy and classical psychoanalysis; DB family therapy was not, contrary to an often-found misconception, a reaction to psychoanalysis, or else how could one explain family therapists (and founders of paradigms in their own right) like Nathan Ackerman or Ivan Boszormenyi-Nagy, both of whom can be regarded as psychoanalytic in the larger sense?⁹ Ackerman and Boszormenyi-Nagy responded to certain anomalies in psychoanalysis but neither rejected psychoanalysis in toto (as have some of the behaviorists) nor did they establish their paradigms in reaction to it as a system.

Neither, however was DB family therapy independent from psychoanalysis. DB family work was related to psychoanalysis in that the

⁹ By this I mean that both deal with intra-psychic processes as well as family structure and dynamics; both are comfortable with psychoanalytic concepts, though they are clearly not limited to them; and both deal with the development and phenomenology of the individual as well as the family system, unlike more strictly "systems" family therapists. Neither, however, should be regarded as psychoanalytic in the strict, i.e., classical or interpretative sense, by virtue of their emphasis on relational processes, as well as intrapsychic, and not the least, by their being founders of family therapy DMs.

latter provided the anomalies upon which the DB paradigm and practice were built. The DB group responded to certain anomalies in individual psychoanalysis, and not to the framework at large. For instance it was not the case that psychoanalysis "did not work," because usually, it clearly did and was successful both conceptually and clinically. The Kuhnian analysis allows us to perceive the connection between DB family therapy and specifically where psychoanalysis did not work, under what circumstances (extension), and why (concept of anomaly). For instance, extension to borderline patients produced anomaly (discrepancy between paradigm-induced expectancy and the empirical findings) with respect to the psychoanalytic concepts of narcissism and transference.

Relationship of the relational DM to both family therapy and classical psychoanalysis. The analysis also helps to clarify the relationship of Harry Stack Sullivan and the relationalists to both psychoanalysis and family therapy DMs. The relational DM, founded by Sullivan, originated in different circumstances than the early psychoanalytic framework, then developed parallel to the classical psychoanalytic line of development. With the extension of classical psychoanalysis, problems in clinical work impelled the more adventurous of the psychoanalysts to cast about for assistance in dealing with these difficulties and many of them encountered the Sullivanian interpersonal approach. By the 1940's, when anomalies were apparent (at least among those analysts treating the new clinical populations), Sullivan's system was well articulated, and

clinically successful, and Sullivan himself was at Chestnut Lodge, already an influential psychiatric facility.

A Kuhnian analysis in DM terms allows the observer to identify those analysts and therapists who are relationalists, and to differentiate them from classical analysts and other therapists. The criteria of differentiation remain the paradigms regarding how to effect cure. Keeping the criteria in mind, it becomes clear once again that the DB family therapy paradigm was revolutionary and responsive to problems in the classical interpretationist DM, and not the relationalist's.

Paradigmatic borders of applicability: disconfirmation versus anomaly.

The Kuhnian analysis highlights another important, and related point. The problems in individual psychoanalytic treatment discussed here, were not a disconfirmation of the psychoanalytic paradigm, DM, or clinical practice. They were not areas of error or mistake or theoretical clumsiness in an otherwise adequate framework. The framework remains confirmed in a number of theoretical aspects over almost a century of clinical work. The anomalies occur only in areas of extension to new phenomena and only during that process of extension. Anomalies occur as the result of a framework being pushed too far; essentially, they identify the phenomena beginning with which a particular paradigm and DM do not work. It would seem that each paradigm and DM must have such a "border of applicability" that in time and extensions, becomes increasingly obvious and eventually acknowledged. A Kuhnian analysis allows differentiation between disconfirmation at the heart of a

framework, and anomalies at its periphery. Both processes eventually result in the replacement of at least parts of the framework, but the route is very different. Disconfirmation can occur at almost anytime; but anomaly can occur only after the paradigm has proved itself successful and was then extended to new phenomena.

Summary. In short, evaluation of a modified Kuhnian analysis would indicate that it is enormously useful historically, sociologically, and intellectually. The modified Kuhnian analysis allowed a productive interpretation of the events surrounding the emergence of family therapy; it provided a potentially helpful form of interpretation for certain sterile controversies, and the analysis highlighted several important features related to the group structure of scientific activity.

CHAPTER VIII

PROBLEMS, REQUIREMENTS AND USES OF THE KUHNIAN ANALYSIS FOR PSYCHOLOGY'S FELT CRISIS

In the preceding chapter, a revised Kuhnian analysis was said to be helpful in a number of ways. In this chapter, two important deficits in the Kuhnian schema will be discussed, and two proposed solutions presented. Then some conclusions will follow.

"Invisible Colleges" and the Coherent Group Structure of Scientific Activity

As was pointed out in Chapter II, although Kuhn's concept answered several of the criticisms which had been levelled at his overall schema, it introduced some new problems in turn. Of particular importance, Kuhn's DM statements merely implied rather than defined, the size of a DM. Two sorts of boundaries are required to determine the "inclusive perimeter" of a DM: one sociological and the other conceptual (or structural). Neither the size (structurally, conceptually or sociologically) nor the boundary criteria were explicated by Kuhn.

Thus, Kuhn was not quite clear as to whether the DM was constituted by the adherents to the same paradigm(s) (a sociological criterion) or by the network of commitments (a conceptual or structural criterion). My strong impression is that he favors the latter; hence, he explicated the internal structure and constituents of a DM (i.e., the paradigms, symbolic generalizations, models and values, as well as habits, instru-

mentation, etc.) and also specifically referred to intellectual habits and the network of commitments. For the purposes of this dissertation, his conceptual/structural meaning (i.e., the "network of commitments" sense of the DM concept) was used as the DM. When using this version a "boundary" is available which helps to differentiate one DM from another and within which a single paradigm (or paradigm set) may be elaborated. This boundary was introduced earlier as the "border of applicability."

Border of applicability. Such a "border of applicability" is established by the gradual emergence of anomalies as the paradigm is elaborated and extended to new phenomena. With successful extensions, that is, where empirical findings are consonant with paradigm-induced expectations, the DM constituents remain productively applicable and the DM is extended. When the extensions continue "into" new phenomenon-areas where anomalies begin to occur, that is, where there occur obdurate discrepancies between paradigm-induced expectations and empirical findings, then the DM framework is no longer applicable or helpful and a gradually recognizable limit is reached. As the DM matures and is extended by adherents to new areas then a boundary will begin to be discernible, to those who think in such meta-scientific terms, and the real limits of the paradigm DM will become obvious.

If a DM is visualized as a framework extending either as a circle (or sphere), or as a "fan-shape" from the paradigm as focus, then the

"border of applicability" will be either closer or further depending upon the congruence between the characteristics of the phenomena and those of the paradigm and DM. Not all groups of phenomena would lend themselves as easily, or as poorly, to a particular DM. For example, with respect to Freud's classical paradigm, extensions to other neuroses from hysteria, and to psychotic depression reached the borders of applicability much later than in schizophrenia. Almost as soon as schizophrenia was investigated by the classical psychoanalytic DM, anomalies began to emerge. With respect to the borders of applicability, when the DM extended into the area of schizophrenia, the border or limits occurred quite soon and "close" to the paradigm. Viewed in this way a DM seems amoeba-like, gently extending its boundaries and finding some areas more congenial than others. Structurally, the limits of a DM's applicability become discernible with the emergence of anomalies.

There are no criteria in Kuhn for the sociological size of a DM. In what ways can the number of DM adherents be ascertained and where is the limit set by which one can assign individuals in or out of a DM? Taking particularly a paradigm as the focus, and surveying a geography of researchers, at what point from that paradigm is an individual a DM member? How directly must s/he work with the paradigm to be functioning within a particular DM? At what point do the problems, methods and solutions of that researcher cross over into a different DM? Kuhn's analysis requires answers to these questions, but does not include any starting points. A series of studies about coherent group structure

in science, however, provide this element and is not incompatible with Kuhn's analysis.

Coherent group structure. Recent work by Griffith and Mullins (1972) provide a necessary supplement to the Kuhnian schema.¹ Griffith and Mullins (1972) and Small and Griffith (1974) have investigated small, very active scientific research groups that had proved scientifically "successful." Based on Crane's (1972) work on "invisible colleges" where she "mapped" the structure of some specialties, Griffith and Mullins (1972) contend that major scientific changes are generated within small, "socially coherent" groups with similar characteristics. They differentiate between the "loose networks" that appear normal for science and the occasional formation of small socially coherent groups that formulate "radical conceptual reorganization(s)" (p. 960) of their field.

Loose networks. Across these loose networks, different groups used a variety of means to facilitate communication. Hence, one research area, speech perception, was small enough that few communicational problems arose despite the relatively low level of social organization (p. 959). In other groups, conference series and exchanges of papers before pub-

¹This is not to imply that Griffith and Mullin's material must necessarily be subsumed within Kuhn's; my impression is that it can well stand on its own. However, since it does meet a perceived need in the Kuhnian analysis very well, I will present it with special reference to this problem of the "inclusive perimeter."

lication met communicational needs (p. 959). However, Griffith and Mullins found that the adoption or development of a particular pattern of communication within these specialties was determined, not solely by the structure of any particular group, but also by the problem it was currently investigating. (The importance of the problem under investigation clearly provides a point of compatibility with Kuhn's schema which also emphasizes problems and their solutions.)

For example, psycholinguists seemed to develop different patterns of organization depending upon whether they were in the process of applying psychological theories and methodology to studies of language (as they did after the development of generative grammar). By contrast, research in the effects of drugs on behavior formed a small specialty in which communication patterns reflected the activities of individual researchers, and the membership of groups in close communication changed continually in response to changes in research interests. This group was particularly well served by journals; thus informal contacts were not supplementing or replacing publications. (1972, pp. 959-960)

Griffiths and Mullins (p. 960) regard these loose networks of researchers as resulting from "'normal' scientific activities" and thus conforming generally to the conventional scientific wisdom of objectivity and emotional neutrality.² They mention that a Kuhnian might regard such groups "as working to fill out existing paradigms" i.e., to be DMs. This appears to me to be a legitimate identity to make.

² A Kuhnian analysis would of course disagree regarding their point about neutrality and objectivity.

Socially coherent structure. Griffith and Mullins (1972, p. 960) further differentiated between the loose networks of normal science, and more tightly knit groups that appear to be essentially revolutionary. They studied six of these groups: the phage workers in biology, Skinnerian psychologists, the quantum physics group in Copenhagen, the Goettingen mathematicians, the audition researchers in psychology and the ethnomethodologists in sociology (p. 960).

All six groups achieved a "radical conceptual reorganization within their field." The members of each group were convinced that they were achieving either the overthrow of a major position in their field, or making a major revision in methodology. In fact, each of the six groups did offer a distinctively different theory or methodology which both countered the currently dominant approach and became influential in its own right. Each group maintained its beliefs over a protracted period and each eventually demonstrated substantial achievements. According to Griffith and Mullins (1972, p. 960) none of the groups consistently maintained the attitude of "disinterested objectivity that is regarded as a norm of science..." In fact, these groups often entered the politics of science to further their beliefs by obtaining or protecting appointments and research supports. Finally, each group operated through "... close and continual interaction..."

...when an audition researcher was asked whether he and others in his field exchanged preprints (prepublication copies of papers), he said that such exchange was usually unnecessary because they followed one another's work so closely that often a single, newly found constant sufficed

to inform others of an important advance. (1972, p. 960)

Internally, all six groups were characterized by the presence of an acknowledged intellectual and organizational leader (or leaders), a geographical center, and a brief period of comparatively intense activity (p. 960). For instance, in quantum mechanics, Bohr was both the intellectual and organizational leader; the geographical location was Copenhagen and the period of intense activity from 1920-1934 (inclusive) (p. 961).

Among the Skinnerians, Skinner was the acknowledged intellectual leader, while organizational leadership was assumed by what Griffith and Mullins refer to as a "cadre of students and postdoctoral fellows" at Harvard. The period of intense activity occurred at both Columbia and Harvard from 1947-1960. (Palo Alto was clearly the DB's geographical center.)

The intellectual leader was conceptualized as laying the original conceptual foundations for the work, as making public statements of theory and research, resulting in an acknowledged theoretical break, and as approving or validating other members' work (p. 961). The intellectual leader was largely responsible for establishing the innovative conceptual base. Clearly in the DB group, Gregory Bateson fulfilled these roles and can be regarded as the intellectual leader.

The organizational leader in all six groups was a respected researcher in his own right. (e.g., Klein in the Goettingen mathematicians with Hilbert and Minkowski as the intellectual leaders.)

The organization leader arranged times, funds, and facilities for research and means for communicating findings and ideas. He arranged appointments in such a way that specific scientists obtain jobs in specific locations, organizes research programs and obtains funds, and guides the organization of meetings. (1972, p. 961)

Internally, the organizational leader functioned to maintain scientific activity within the group's program. Griffith and Mullins (p. 961) felt that their findings generally indicated "a conscious effort to direct the group's work toward a specified series of problems, from a particular perspective, with a stated goal." (p. 961) In Kuhnian terms, the organizational leader could be said to maintain the focus on the elaboration of the paradigm, within DM boundaries (with regard to models, values, etc.). An inference could be made that while the intellectual leader could be primarily credited for the paradigm (as s/he established the innovative conceptual base), the organizational leader could be credited for maintaining and guiding the DM, in both the sociological and conceptual aspects. Thus, the organizational leader, according to Griffith and Mullins, arranged appointments, conferences, and research programs - fostering the research activities. At the same time, s/he would maintain that activity within paradigm-directed boundaries, e.g., the organizational leader functioned with a "conscious effort to direct the group's work toward specified problems, from a particular perspective, with a stated goal." (1972, p. 961)

It appears to me difficult to tell which of the central DB members fulfilled the role of organizational leader. In later years (after the

1962 dissolution) Watzlawick filled this position in organizing conferences, publishing reviews and editing compilations. More personal, and/or detailed information regarding the internal organization and roles of the DB DM would be needed to accurately ascertain who, and whether, there was an organizational leader(s).

Each of the six groups established theoretical and sociological boundaries and provided identity and in-group versus out-group lines (p. 960). (Thus, for the operant conditioners, the Hullian learning theorists were the out-group. For DB members, the individualistic psychoanalysts constituted the out-group). Also, the boundary served to severely limit the range of incoming information regarded as relevant. Griffith and Mullins (p. 961) refer to "their general indifference to the work of other researchers..." which, naturally, helped to generate antagonism.

Each of the six groups also concerned itself with recruitment of new members (p. 962), facilitation of communication (p. 962), and the protection of priority rights of discovery of its members.

There are several points of compatibility and complementarity between Kuhn's schema and the Griffith and Mullins work on "invisible colleges." Both emphasize the importance of "the problem" as a focus for organization of the research group; they emphasize the group structure of science and the importance of the "socially coherent" work group with respect to communication, productivity, originality, and identity. They appear compatible in seeing science as having "normal" and revolu-

tionary phases, with progress alternating between the accretionary and the revolutionary (or "innovative"). Griffith and Mullins make a differentiation Kuhn does not -- between innovative revolutionary and innovative elite but do not identify what is fundamentally different about them.³ Kuhn's schema implies that the revolutionary groups have a truly revolutionary paradigm for a large proportion of the relevant disciplines, whereas for elite groups, the innovative paradigm is not so clearly opposed to the dominant framework in some way (whether conceptually or proportionally to number adherents of the dominant approach).

Finally, both analyses perceive science as conducted in groups which have developmental aspects: inception, intense activity and either dissolution or absorption as beliefs gain adherents and respectability.

Supplementing Kuhn's schema with the work of Crane, and of Griffith and Mullins and Small provides both direct information and also some heuristic uses. For instance, Small and Griffith (1974, p. 35) indicate that the structure of biomedical literature differs from that of the physical sciences, they provide a methodology using citations in publications they had found useful. Taking into consideration their point

³ Elite innovative groups such as Bohr's quantum mechanics groups, were recognized by other members of their field, as being of central importance while they were actively diverging from those other researchers (p. 960). The audition researchers and the Goettingen mathematicians were also in this category. The Skinnerians, phage biologists and ethnomethodologists were perceived by Griffith and Mullins (p. 961) as revolutionary: as both more highly organized, and more clearly opposed to the outgroup. Their criteria appear not as firm as Kuhn's for his revolutionary paradigms.

regarding different structures, one can speculate that the structure of literature in the social sciences may be different than either that of the physical or biomedical sciences.

The "invisible college" work also sheds some light on the unresolved problem of DM "inclusive perimeters" in the Kuhnian analysis. Kuhn does not specify the average "size" or inclusiveness of a DM, nor how closely a researcher must work with the paradigm to be a legitimate member of a DM. Griffith and Mullins' work suggests that revolutionary DMs are rather small in number and that researchers deal with the paradigm(s) rather directly. Normal science DMs appear larger and more diffuse, with more permeable boundaries and less antagonism between in- and out-groups.

Need for the "Meta" DM Concept

Kuhn's formulation of the DM concept brings up a second major problem. This problem is pointed to by two different types of issues. The first concerns the relationship of "clusters" of DM's within the family therapy field. The second concerns controversies that appear to be either methodological or inter-DM debates, which, upon closer inspection, cannot adequately be accounted for by either method or the DM concept.

DM clusters. To take the first issue first: it is obvious in perusing the family therapy landscape that there are a number of DMs. The DB DM is one. Others include the DM developed around the work of Nathan Ackerman in New York City, and that around Lyman Wynne's work in Rochester.

In Philadelphia, Salvador Minuchin's work has been developed into a well-known DM, and I. Boszormenyi-Nagy's work is becoming increasingly influential. Moreover, several other DMs are being, or have been, developed (e.g., around the ideas and practice of Murray Bowen, of Speck and Attneave, and of Bell.)

Several of these separate and differentiable DMs in family therapy seem to "belong with" or cluster with the DB DM. Such a DM would be Minuchin's structural family therapy DM. Though the two DMs are based on different paradigms and use different symbolic generalizations, heuristic models, techniques and goals, there are some affinities between them. Structuralists can be comfortable with DB techniques and vice versa.⁴ Both groups stress problem solving, change, and short-term work rather than insight, explanation, and "deep" change. Both groups exclusively focus, in conceptualization and clinical practice, on the system and not with the individual. Yet adherents from each group would bristle at being asked to cross their respective DM boundaries into the other DM, and their own DM identities are firmly maintained. Kuhn's DM concept can neither explain the ways in which these two DMs cluster with each other, nor why this cluster is so far removed from another found in the family field.

⁴In fact, after the dissolution of the DB DM and Minuchin's establishment of his DM, Jay Haley left Palo Alto and joined Minuchin in Philadelphia. They apparently worked closely together for some time, then parted their association, neither one of them "crossing" into the other's DM, and both continuing after the separation, to exemplify and develop their respective DMs, Minuchin staying in Philadelphia and Haley going to Washington, D.C. (Minuchin, 1974)

This second cluster of DMs includes Ivan Boszormenyi-Nagy's DM in Philadelphia, Ackerman's in New York, Helm Stierlin's in West Germany and R.D. Laing's in Great Britain.⁵ All of these DMs deal with the integration of the family system and the individual, and allow phenomenology in their practice and conceptualization. All these groups emphasize understanding, insight and explanation in relation to change; also, treatment tends to be longer than in the first cluster and is directed toward "deeper" changes. There exists a good deal of acrimony about this issue; the DB and related DMs deride Nagy, Ackerman, etc., as being psychoanalysts, while this second cluster usually regards the techniques of the DB and Minuchin cluster to be conducive to only the most superficial of changes. Also, this second cluster will occasionally address larger issues, (e.g., role of childhood, adolescence in the 20th century, changing role of the family, etc.) while the first cluster will almost never engage in such discussion. Those in the second cluster often have their conceptual roots in psychoanalysis (e.g., Ackerman, Boszormenyi-Nagy), though they are obviously family therapy and not psychoanalytic DMs.⁶

⁵ Although I am not very familiar with Norman Paul's work regarding unresolved grief reactions across generations, it seems that his DM also belongs here.

⁶ It should be pointed out that the conceptualizations and values show a relationship to psychoanalysts; nothing is being implied here about the training of the therapists before they became family therapists. Thus, Salvador Minuchin and Donald Jackson were both trained as analysts. Minuchin's paradigm and DM, however, are not related in

Kuhn's DM concept, unfortunately, cannot explain the basis under which these clusterings occur, nor what makes the two clusters so obviously disparate from each other.⁷ What is obvious is that some "meta"-DM concept is required--one which provides both the criteria of inclusion within cluster and the criteria of disparity between DM clusters. For instance, about what kinds of things do the intra-cluster DMs agree? One example might be that the DMs must agree with regard to metaphysical models; while Kuhn has included both metaphysical and heuristic models as DM components, I would regard heuristic models as appropriate to the DM concept level (e.g., cybernetics as a heuristic model for the DB homeostatic aspect of the paradigm), but metaphysical models as inappropriate to that DM level. The other DM components are rather narrowly focused: thus, "values" relate, for Kuhn, primarily to good theory construction. Heuristic models, like cybernetics, are appropriate at this DM level. Metaphysical models fall outside this type of narrow-

concept, practice or concerns with Ackerman's or Nagy's, but is related to the DB's. Apparently when Minuchin formulated his paradigm, he "left behind" the psychoanalytic thinking. Jackson, on the other hand, did not. Though he formulated the DB, his continued adherence to his former training eventually resulted in a split between him and the mainstream DB formulations; for instance, his insistence on the legitimacy and necessity of phenomenological issues is similar to a psychoanalytic position, but resulted in the 1959 DB DM split. This insistence upon phenomenology is also present in Ackerman's and Boszormenyi-Nagy's work.

⁷I am not implying here that there are only two of these DM clusters in the family therapy field; there may be several more. These two are the only ones I have discerned.

focus conceptualization. The function served by DMs do not use or require metaphysical models. They belong probably to that "meta"-DM level; a further point of inquiry should focus on a comparison of metaphysical assumptions across the DMs within one of those large clusters and then between these large clusters.

Another issue that suggests the need for a meta-DM concept concerns ostensible methodological or inter-DM controversies which, upon close inspection, are actually debates about typical and repetitive inter-cluster issues which are fundamental and often pre-logical. An example of this type of "meta-DM" controversy took place within the DB DM in the guise of a methodological debate, and led directly to the 1959 split.

"Meta"-DM debate. The debate very probably occurred in late 1956 or early 1957. Haley (1976, pp. 72 and 75) states that early in 1956, the project shifted to emphasize actual family behavior and that differences surfaced with the pressure of dealing with actual families; also, he states that these differences in approach did not begin to appear in publications until 1958, "the next year that project members published papers." (p. 75)

It became obvious to the project members that there was a large gap between their conceptualizations, particularly the double bind idea itself and the raw data of family interaction.⁸ They felt that some

⁸ It might be recalled here that the DB paradigm had been formulated deductively; as such, when the group came to "test", compare or apply it to actual data, some degree of bridging material would almost inevitably be lacking. It is probably only a point of curiosity but, if all

type of theoretical model was necessary to describe both family patterns during conversation and also those characteristic patterns of behavior of families which contained a schizophrenic member. They agreed that the "theoretical models and terminology of psychoanalysis and other psychological approaches were inadequate for the problem"(Haley, 1976a, p. 73), but began to disagree upon what sort of approach to use from there.

A difference in view in the project which had been inherent in the work from the beginning became more evident. There had always existed in the project a controversy over what aspect of communication to focus upon and what terminology and theoretical models to use. A schism developed between (a) a strictly-communication approach dealing with the description of observable messages in terms of Logical Types, and (b) an approach which emphasized the codification of messages, or the internal processes of the receiver, and centered on perception and learning.⁹ (Haley, 1976a, p. 73; emphasis added)

The debate centered upon the paradigmatic "levels of communication", particularly with respect to the message and its qualifier. According to Haley's account (1976a, pp. 73-75), the "internal processes wing" argued that the distinction between a message and the codification of a message by a receiver was fallacious. Later they argued that the

revolutionary paradigms are developed deductively, as Kuhn has implied, is there always this difficult point of convergence, when empirical material is brought to bear on the idea? This process certainly occurred in relation to the theories of relativity forwarded by Einstein, whose paradigms had been deductive. Whether this is also the case for less widely known paradigms is unclear.

⁹ The first of these approaches was labelled the "behavioral wing" and the second the "internal processes wing" or the "higher generalizations wing."

terminology should be maintained at a higher level of generalization than the mere description of messages. The terminology of this wing included such concepts as "learning, perception, awareness, expectation and the language of emotions." (p. 73) Important questions for them were whether the receiver of the messages were aware, whether s/he was misperceiving or misinterpreting, and what his/her experience might be "was he suffering trauma, was he experiencing grief, was the experience hurtful, and so on." This wing objected to restricting the use of the DB to specific interpersonal situations and preferred to use the concept quite broadly; thus, they felt the term to be applicable to evolutionary processes and telencephalization of the brain as well as an interactional pattern in families conducive to the development of schizophrenia. From the positions argued by this internal process wing, it can be inferred that it was comprised of Jackson and very probably Bateson.

Haley was clearly a member of the "behavioral wing."¹⁰ This wing believed the preferable approach to be the study of "strictly observable messages" (1976, p. 74), and argued that an "age old problem of psychology" could be avoided if they studied only those things which could be directly observed and verified. This wing believed that the concept of levels of communication made it appear possible, for the first time, to develop

¹⁰ Once again, Weakland's position is more difficult to discern here. He might not have had a firm position but rather may have served as a mediator and helped the DM to produce as much as it did from the split in 1959 to dissolution in 1962.

a systematic description of human behavior. To continue to deal with "metaphoric statements about the internal processes of the individual, even if conceived in a different way" (p. 74) was perceived as leading the project back to psychology's preoccupation with the individual, and away from its identity with the study of interactive processes. It was argued that the more the focus was upon the interchange of messages, the more complete the description.

If it is said that if a schizophrenic faced with certain messages misinterprets or misperceives them, the description must end there. If his response is also described in terms of messages, the descriptive system becomes more complete. The more highly generalized the definition of terms, such as the double bind, the more difficult it would be to develop a systematic description which could be applied to the data.¹¹ (Haley, 1976a, p. 74)

The internal processes wing argued that the behavioral wing was too narrow in its approach, and the behavioral wing that the other was too diffuse and ambiguous to have useful application to actual data (pp. 74-75).

The two wings differed with respect to: type of approach, admissible types of data, degree of generalization for the DB concept; also whether certain processes fell into their own, or the other, wing. Thus, they debated upon where "learning" belonged: the behavioral faction argued

¹¹ It should be recalled that Haley's abiding interest during and after the project was the systematic classification of families by communicational types.

that the term "learning" was not a communication term for descriptive purposes, while the internal processes group "argued that it certainly was and should be used in a description of the data." (Haley, 1976a, p. 75)

It is apparent that Haley recognized these differences as very serious and perhaps fundamental, certainly more than methodological. Haley remarked that these differences were inherent "from the beginning" (p. 73) of the project, and that the positions taken antedated the DM itself. These positions were increasingly reflected in the work in progress when it was subjected to the pressure of direct empirical application.

Methodology develops within a DM and is related to the DM's other constituents. Arguments about methodology can be resolved within a DM as the adherents share other DM constituents, especially the paradigm, and as such have a shared context, set of goals, terms, etc., through which to argue the specific differences of method. It is specifically because method is not independent of DM that inter-DM methodological debates are fruitless. Each side is arguing a DM-dependent methodology to another DM-dependent methodology as though the methodologies were DM-independent.

In the present case of the behavioral vs. internal processes debate, the differences stem not from different DMs, but rather from antecedent and fundamental differences in conceptual-philosophical orientations, which will be designated as "meta-DMs" (MDMs). Questions regarding whether behavior or phenomenology is more important, and

which can be studied and why, are answered at the MDM level and not at the DM level of symbolic generalization, heuristic models, paradigms and values regarding theory construction. The choice between behavior and phenomenology occurs because of prior adherence to a MDM. Haley identified their differences as predating the project, as inherent in the position they took, and as persistent through time.

These are the same MDMs alluded to in the foregoing discussion of "clusters" of DMs. The clusters of DMs occur within one MDM, and the two MDMs provide the criteria for inclusion and the boundaries between those two family therapy clusters.

Some preliminary MDM constituents. Some preliminary constituents of MDMs can be identified by examining what is shared by DMs within their respective MDM's and also by examining the areas of consistent differences between the two identified MDMs. It should already be clear that the MDM constituents would have to include both explicit and implicit aspects, and both intellectual and attitudinal components.

In the following discussion the cluster of DMs formed by the DB DM and Minuchin's DM will be referred to as MDM I; the cluster of DMs formed by Boszormenyi-Nagy's, Stierlin's and Ackerman's DMs will be referred to as MDM II.¹² Reviewing the DM clusters several patterns

¹²This is not to imply that other family therapy DMs do not or cannot fall within one or another of these MDMs, or some other MDM.

emerge.

Philosophical orientation. First, the DMs within MDM I ascribe to a logical positivistic orientation, whereas those DMs in the second MDM all ascribe to a dialectical orientation.¹³ This difference in philosophical orientations corresponds to differences in what Radnitzky has termed the Anglo-Saxon intellectual tradition (for logical positivism) and the Continental intellectual tradition (dialectics). (Parenthetically, the Continental tradition in this country is markedly less powerful than the Anglo-Saxon, and most of the DMs which adhere to it occupy a counter-dominant position in their fields. Thus Boszormenyi-Nagy's DM is counter-dominant to Minuchin's in terms of influence, number of adherents and to a certain degree, their perception of their own positions vis-a-vis each other. The way in which these two MDMs conceptualize what the problem is and what their goals are, is heavily influenced by their (usually implicit) adherence to either positivism or dialectics. One of the constituents then differentiating the two is a difference in philosophical orientation. This point receives some support if one recalls the reservations mentioned earlier regarding metaphysical models. My strong impression was that, while heuristic

¹³For instance, with respect to MDM II, see: Laing and Esterson's Sanity, Madness and the Family (1964); Boszormenyi-Nagy and Spark, (1970); and Stierlin (1968) for family therapists who could be considered to cluster in MDM II; for MDM I family therapists, see Haley (1973), and Minuchin, 1974.

models were indeed constituents of DMs, metaphysical models had no place in the DM as narrowly defined by Kuhn and Masterman and as used in the present analysis. There is obviously a similarity between the ideas of "metaphysical models" and of "philosophical orientations", and they appear to occupy the same, more abstract level of thinking than the heuristic models. For this reason, heuristic models appear appropriate to the DM level of conceptualization and philosophical orientation (and metaphysical models) the MDM level.

Value systems. Secondly, they differ with respect to the values to which they adhere and these values are consonant with the philosophical orientation of the respective MDMs. A particularly strong contrast in values between the two MDMs corresponds to the differences between predictive understanding and interpretive understanding. Thus, MDM I values prediction, control, results as measured by change in family structure (either as expressed in communication or subsystem coalitions and splits). MDM II on the other hand values understanding, insight, and dialogue and what Radnitzky (1973) has called an "emancipatory interest."¹⁴

Thus, DB and structural family therapies have been criticized by

¹⁴An "emancipatory interest" is usually defined as a concern with the forces influencing or acting upon an individual in his/her social and economic context; the well-being of the individual is of concern.

non-adherents for maintaining the oppressive status quo in some families and for the fact that it is not at all uncommon for them to reinforce cultural, social, or gender stereotypes in what they state is in the service of change. They are often criticized by the MDM II adherents for making only "surface" changes which either back-fire or dissipate; MDM I adherents claim that the MDM II therapists are really psychoanalysts and in their pursuit of "deep" changes, require either a select population of families or lose many families to premature termination. In general, MDM I therapists use a rapidly moving short-term approach while the MDM II prefer longer treatment.

Focus of attention. Related to the above two differences, these two MDMs differ with respect to their foci of attention or their choice or level of phenomena. Thus, MDM I DMs focus on behavior while MDM II emphasizes (though not exclusively) phenomenology. Similarly, the DB and Minuchin's group focus exclusively on the family systems level for both conceptualization and clinical practice whereas MDM II groups distribute their attention between family systems dynamics, individual phenomenology and the integration of the two. In the latter MDM there appears to be an attempt to balance the roles, prerogatives and obligations of the individual with those of the family. In the first MDM, the individual, particularly with regard to his/her phenomenology is actively ruled out, e.g., the "internal processes" vs. "behavioral" debate in the DB group. The "behavioral" wing prevailed and the DM

remained in the same line of development in which it had started.

This issue raises an important point. How, if both MDM I and MDM II were represented in the DB DM, could the DM function and survive? My reply would be that it could not. This was the main issue over which the group debated and why Jackson eventually established his own group at MRI (the Mental Research Institute). Jackson's orientation was obviously toward MDM II while the dominant group orientation was MDM I. While Jackson contributed to the paradigm, it should be recalled that his contribution, the homeostatic aspect, was consistently downplayed, and the communicational aspect emphasized and more fully elaborated. My impression is that the newly emerging paradigm and DM could not "afford" to elaborate the homeostatic aspect because it implied a return to traditional individual psychiatric thinking rather than the family-systems viewpoint. If the DB DM had not subdued or de-emphasized the MDM II component and instead elaborated its dominant MDM I aspects, my impression is that it would not have been revolutionary--not because the MDM I Gestalt is revolutionary, but because their paradigm within it was. As it was, Jackson joined the DM in 1954 and the debate erupted only two years later; this set of differences between the two MDMs is best exemplified in the behavior vs. phenomenology issue, the set of differences Haley refers to as "inherent" from the beginning.

In short, a DM cannot be split between two MDMs and any elements of the counter-dominant MDM must be vigorously subdued, particularly in the early paradigm-DM stages. Griffith and Mullins (1972) refer to

this suppression function from another angle; they indicate that one of the functions of socially coherent research groups, and especially one of the leaders, is to suppress tangential or alternate lines of investigation within the group and to provide a relatively impermeable barrier to such information from outside the group. Research generated within or by other MDMs than that MDM dominant in a particular research group is precisely the sort of research which "should" be subdued, particularly in early phases of the DM; its presence is divisive in the extreme, as the fundamental belief systems of any two MDMs are different. To be articulated, a paradigm requires a period of uncontroversial and consensual effort, and this implies the operation of only one MDM. If the two clusters of family therapy DMs are any measure, MDMs differ sufficiently to effectively preclude concerted effort for any reasonable period of time.

Models of explanation. A final discernible constituent of MDMs is a preference for a particular model of explanation. The MDM I DM's characteristically preferred mechanistic explanations and conceptualizations. The fact that they deal with family systems should not be taken to mean the MDM I DMs deal with "open" systems; the characteristic system found in their conceptualization is the simple, closed system. ("Closed" systems are usually defined as those systems with no incoming or outgoing elements, whereas "open" systems exchange elements, across the system's boundary, with the environment). Their systems

approach is limited to the cybernetic model of closed rather than open systems and while this is a far cry from the linear thinking of their predecessors, it is also a far cry from the conceptualization required for the open system. It is the second MDM that prefers the open systems as a model of explanation. This latter group characteristically will deal with the system formed by family-individual and individual/family nexus, as well as the individual-family-society configuration.

The MDM gestalt. It becomes increasingly clear that the constituents within each MDM are closely related. For instance, predictive rather than interpretive understanding is often associated with logical positivism, which is in turn usually associated with a preference for mechanistic types of explanation. Similarly, many of the MDM II characteristics are often associated with each other, e.g., dialectics, phenomenology, an interpretive understanding and an emancipatory interest. What this suggests is that the constituents of MDMs are neither random nor independent. They probably function with some degree of integration and certain characteristics are much more likely to be found in concert than others. For instance, though probably not necessarily impossible, it seems that predictive understanding (as a value) is much less likely to be found with phenomenology (as a focus of attention) or with organismic types of explanations. However, the combination of predictive understanding, focus on behavior, and mechanistic explanation is more likely, makes "intuitive" sense, and is in fact encountered, for ex-

ample, as MDM constituents for behaviorism.

Though arrived at inductively from a review of family therapy DM clusters and though preliminary, the constituents for the MDM concept fit each cluster and appear to "fit" with each other. As a preliminary hypothesis, MDMs could be said to be constituted of:

1. a philosophical orientation
2. preferred model or type of explanation
3. a value system, and
4. phenomena of focus (focal phenomena of observation)

Independent corroboration for the presence and internal coherence of such conceptual constellations comes from such work as Coan's (1963). Factor analysis of characteristics of important works in the field of psychology demonstrated six continua, which split up into three sets of complementary, or antagonistic, pairs. The first pair of characteristic tendencies in these major works was termed a synthetic vs. analytic approach. The synthetic continuum was characterized by a subjectivistic, holistic, and qualitative approach, whereas the analytic continuum was characterized by what was seen as an objectivistic, elementaristic, and quantitative trend. Coan regards this pair of approaches as roughly equivalent to Allport's Leibnitzian and Lockean categories.

The second pair of antagonistic approaches was a functional vs. structural dichotomy, roughly exemplified by William James and the experimentalists respectively. In Coan's study, the functional approach was comprised of a dynamic personal approach which emphasized internal

or biological sources of behavior, whereas the structure's tendency emphasized a static, transpersonal approach. Finally, a weaker, yet still distinct pair of continua was the fluid vs. restrictive tendencies.

Along the "fluid" continuum there was a "basic" predisposition

to experience people and life in all their complexity in a rather relaxed fashion while the latter suggests a tendency to deal with reality in a more controlling and compartmental fashion, through restriction of attention and through isolation of entities and events. (1973, p. 240)

Coan's work demonstrates the presence of the tendencies identified in the MDMs. Clearly, Coan's analytic, structural, and restrictive tendencies would be associated with the MDM I DMs, and their predilection for a logical positivistic orientation, mechanistic models of explanation and predictive understanding. On the other hand, the synthetic, functional and fluidity characteristics are associated with MDM II and their dialectical, phenomenological and organismic preferences. What is particularly useful in Coan's work is the demonstration of the presence of these characterizing tendencies, their non-independence from each other, their close correspondence with the MDM constituents and finally, their arrival by inductive means.

The MDM constituents were arrived at primarily deductively; the DM clusters were examined for the aspects which allowed certain DMs to cluster and those that differentiated the clusters. Then, these criteria were used as MDM constituents. Coan's continua, however, were arrived at largely inductively in a factor-analytic study. As such, very similar conclusions were reached from two separate directions and

this underscores the substantiality of these tendencies and the importance of investigating them further.

Moreover, these tendencies were found by Coan to occur in several areas of psychology and the present dissertation found them to occur in family therapy as well. It is apparent that these divisions or continua hold for areas besides family therapy. If these continua, and MDMs are appropriate for other areas as well, this would imply that such a Kuhnian analysis which included the MDM concept, could appropriately be applied to an endemic, dialectical tension in psychology.¹⁵ While the Kuhnian schema implies some sort of progression (if not necessarily progress) or evolution of DMs, from one to another through time, the MDM concept and Coan's work imply a continuing dialectical tension between MDMs. There may be no progression from MDM to new MDM, but rather the persisting tension between constellations of belief. It may be that, in the social sciences, this conceptual structure persists and should be recognized.

MDMs as "constitutive realities." MDMs could effectively function as what the ethnomethodologists term "constitutive reality." (Mehan and Wood, 1975) Briefly, ethnomethodology is described by some of its adherents (Mehan and Wood, 1975) as a reality system that investigates

¹⁵ My thanks to Dr. Park for pointing out this important MDM aspect to me.

those common features of all realities. A central thesis is that realities do not merely occur, they are constructed and maintained by ceaseless activity. These constitutive realities are based on in corrigible propositions, on "unquestioned and unquestionable axioms" (Mehan and Wood, 1975, p. 9) which establish the facticity of the world for the holder of the proposition. The incorrigible propositions of a reality system serve as criteria to judge other ways of knowing (1975, p. 14). Thus, if an individual knows the world in mystical terms, s/he cannot be convinced or persuaded out of that reality system, and in fact, many of the arguments used in the attempt will be re-constructed by the individual into mystical terms, which process functions as additional support for the incorrigible propositions. In other words, the arguments will be translated into the person's reality system and as such, will serve to support rather than relate his/her particular reality system.

The alternative reality is filtered through the individual's own reality system, which is effectively incompatible with the alternative, and the reality work justifies the incorrigible propositions, upon which it rests. Mehan and Wood (1975, p. 12) see this as a self-preservative reflexive process which occurs in oracular, scientific and commonsense reasoning. All the features of reality systems discussed by Mehan and Wood (e.g., coherence, interaction, fragility and permeability) are maintained reflexively; so, for example, research questions are asked in terms of the system's incorrigible propositions and investigated with methods of that system, which "prove" the

facticity or existence of the reality system itself, regardless of whether the hypothesis is confirmed or disconfirmed. MDMs function as reality systems; with their philosophical orientations, models of explanations, values and relevant phenomena, MDMs define, delineate and constitute a reality system for the scientist. MDMs provide the basis for what is perceived, and what is "known" to exist in the world; as well, they provide a preferred, correlated type of approach or explanation and the values within which to negotiate. Further, MDMs display the same features Mehan and Wood attribute to reality systems, e.g., internal coherence, interaction, etc.

The contention that MDMs function as reality systems for scientists implies that often, or usually, their effect is implicit and they can function both before and after the development of a paradigm.

MDMs can both precede and follow paradigms. As Masterman has pointed out, something precedes the paradigm; it is my impression that it is the MDM that precedes the paradigm. An individual, or set of individuals, perceives a particular problem through the filter of his/her MDM and is able to arrive at a problem-solution. As was the case with the DB paradigm, the revolutionary problem-solution was reached by a group that approached the problem with a different MDM than had been used.¹⁶ By essentially re-constructing the problem, it is able to be

¹⁶ In terms of the two MDMs explicated here, psychoanalysis would appear to adhere to MDM II; it is primarily interpretive, dialectical, phenomenological, etc.

solved. The MDM concept as so constituted would help to explain Masterman's point (corroborated in the DB DM) that revolutionary paradigms are achieved by "rank outsiders"; these paradigm-makers probably adhere to a different MDM, one which can successfully handle the problem at issue. This was the case with Freud. His perception of hysteria in a primarily dialectic framework rather than neurology's logical positivistic, mechanistic approach allowed him to reconstruct or re-perceive the problem and propose a fruitful solution.

This type of situation leads to an interesting question. Is it the case that any change in perception (or problem re-construction) is helpful or is it rather the case that some approaches are more conducive to solving certain types of problems than others? That is, are all MDMs equally helpful with all types of problems, or are some approaches more conducive of solution? For example, Freud approached hysteria with a primarily dialectical MDM¹⁷ and was successful in his problem-solution; prior positivistic mechanistic approaches had not been successful. Is it the case that the opposite change would have been similarly successful: if, after viewing hysteria dialectically with no success, a mechanistic approach were suddenly brought to bear--would just the shift in MDM have been successful? Or is it the case that for certain reasons a dialectical, interpretive, phenomenological approach was more likely

¹⁷For arguments that Freud's conceptualization was primarily dialectical despite his early efforts to inject logical positivism, see D. Bakan's Freud and the Jewish Mystical Tradition. (1958)

to be successful?

It seems difficult to believe that a change in MDM per se is sufficient to induce or allow such a solution; in this hypothetical case of an anomaly with hysteria if dialectics were being used, it is difficult to imagine that a change to a positivistic, mechanistic approach would be helpful. Instead, the constituents of MDM II rather than MDM I seem more conducive to dealing with hysteria. In general, the characteristics of various MDMs appear to make a difference in approaching different types of problems and there is some indication that some MDMs may be more suitable, or more helpful, with some problem types than others.

The idea of a science for meaningful phenomena. The ethnomethodologists can again provide some insight into this issue. One of their central tenets is that some distortion is inevitable in investigating a reality system because one reality cannot investigate another without "running it through its own knowledge and reasoning system." (Mehan and Wood, 1975, p. 70). In particular, the imposition of one reality on another necessarily distorts the reality being studied. (1975, p. 38) During scientific activity, the approach of the scientist imposes a structure on the respondent "that may not be consistent with the respondent's daily life." (1975, p. 62) Mehan and Wood are not objecting to the abstracting from phenomena which they perceive all scientific work as doing, but rather they object to the dissonance between features of the approach and features of the phenomenon under study. Here, they are

considering sociology specifically.

The trouble with sociology is that its abstraction systematically distorts what common sense tells us was the beginning phenomenon of interest: the actual day-to-day social life of human beings. In the sociologist's tables of data, and even more in the theories made up about those tables, one cannot find a sense of the person's daily activities that produced the various phenomena those tables talk about. (1975, p. 48; emphasis added)

Essentially, what is being proposed by Mehan and Wood is that some distortion is inevitable while investigating one reality system through another; there are degrees of distortion. The "fit" between the two reality systems is either more, or less, conducive to distortion. When the "fit" is fairly close between the investigating and the investigated systems, the findings convey a sense of the original phenomena of interest. If the fit is poor, the phenomenon will either be unexplainable, or its sense will no longer be apparent.

They proceed further, by stating outright that ethnomethodologists are agreed that methods borrowed from the "natural sciences are inappropriate for the study of meaningful phenomena" (1975, p. 150). They apparently demand a new methodology, one that is "more becoming to the phenomena realities display" (1975, p. 225). This new sort of methodology retains, or allows, the retention of the "meaningfulness" of the phenomenon being studied; that is, they demand that those qualities the scientist seeks to study in a phenomenon he retained and revealed by the method, not obscured or "controlled out".

If the MDMs are indeed closely related to, or roughly identical to,

these reality systems, the same observations and conclusions would apply. To wit, methodologies imported from the natural sciences excessively distort meaningful phenomena (e.g., dialogue, psychotherapy, psychopathology, social behavior, political practice) whereas methodology designed to take into account these meaningful aspects could be less distorting. If this is the case, then Bateson's point that the DB was an epistemology and was therefore not testable in the "usual" scientific manner is very appropriate. On the other hand, Haley's family experiments attempted to impose a natural sciences MDM or reality structure on meaningful interactions--a process conducive to significant distortion of those meaningful elements.

Essentially, what is being argued is the necessity of an appropriate science for meaningful phenomena. The idea has been presented before (Hudson, 1972; Raush, 1974), and deserves discussion. The MDM concept provides an expression of scientific reality systems in such a way that conceptualization and method are linked to the scientist's reality view. As such, it provides a framework within which to evaluate scientific approaches to see whether they distort, or reveal, the meaningful aspects of human phenomena.

The MDM concept, then meets a number of needs. It helps to explain the basis of DM clustering, and also the differentiation between clusters in family therapy, as well as assisting to unravel some controversies (e.g., "behavioral" vs. "higher processes" debate) and research group splits; it answers, at least in part, the question of what it is

that precedes the paradigm, while also answering Shapere's lament that with revision, Kuhn deleted the paradigm of its "overarching" power in scientific activity. The physical sciences share not an overarching paradigm, as Kuhn and Shapere have implied, but an overarching MDM; specifically, the physical sciences subscribe to MDM I, which is mechanistic, positivistic and prefers predictive understanding. The MDM concept, by thus subsuming many DMs with a number of common characteristics, serves some of these "overarching" functions.

Moreover, it is at this level that the "conversion" experiences occur; Kuhn's critics have been correct in not understanding in what way conversion experiences relate to methodology a paradigm. It does not; "conversion" experiences occur at the MDM level. Conversion at the MDM level has direct implications for the DM used, in which is found concrete and specific methodological functions. Finally, the MDM concept, when joined by the paradigm and DM concepts, can also help to interpret the present day felt crisis in psychology.

Implications for the Present Felt Crisis in Psychology

Essentially, I am proposing that the modified Kuhnian analysis developed here would be helpful in understanding the present felt crisis in psychology if used as a framework of interpretation. Using the paradigm, DM and MDM concepts, and emphasizing the group structure of scientific activity, the analysis would shed light on several facets of the crisis.

The felt crisis has been documented to be a complicated and multifaceted situation, with a history of its own. In general, it can be characterized as a search for guiding and limiting frameworks. Paramount is a sense of impasse, of enormous effort with little advancing knowledge. The lack of long-term progress is a manifestation of this duality; on the one hand, enormous effort has gone into the accumulation of mountains of "facts", upon which few can agree, and even fewer can state to what goals this accumulation leads. Psychology's high technology and advanced methods are often used in the services of ambiguous goals. This sense of impasse is often expressed in controversy. Upon examination, some of the long-standing repetitive methodological controversies will very probably show themselves as amenable to a Kuhnian analysis; some will appear to be searches for an appropriate DM and others for comfortable MDMs. For instance, the psychologists who write of crises of identity for their sub-disciplines will often be drawing attention to the search for a viable DM. The identity of individual scientists and of groups focuses on the problems they address and the approaches evolved to solve these problems. These clearly are paradigm and DM issues, as such, and amenable to a full Kuhnian analysis. On the other hand, other types of controversy appear to point to MDM issues: including such issues as the increasingly elaborated difficulties with the traditional subject-object split in research, or with the observation that a large proportion of significant and original contributions have come from practitioners who have dissociated themselves

from formal psychological research methods. These two crisis issues are clearly related to MDM concerns; when the "scientific method"¹⁸ is being questioned and/or abandoned for alternative approaches, we are seeing individuals change their MDMs. What is particularly striking about these doubts and the occasional MDM changes, is that scientists are making them, not because of persuasive philosophers or meta-scientists, but because their approaches have increasingly proved unsatisfactory to the scientists themselves. As Mehan and Woods (1975, p. 210) point out,

Scientists will not be convinced by opposing philosophies that demand abandoning a form of life that demonstrates its power daily. To tell scientists that their proofs are "only" reflexive accomplishments does not alter the experiential validity of those accomplishments. Science as an activity does not rise and fall on the consistency of its "reconstructed logics".

The fascinating point here is that the scientists themselves have begun, in sufficient numbers to bring about a crisis, to openly doubt the appropriateness of their approach and some are casting about for what they refer to as a "new methodology." What should be obvious by now is that when such a term is used, what is actually being referred to is a conceptual constellation of intellectual and attitudinal components with which certain methods and designs are related. Some of these "new"

¹⁸"Scientific method" here refers to the dominant MDM I approach which, in this country, is or has been virtually synonymous with science itself. Because of the preponderance of MDM I scientists, changes in approach would usually be from MDM II, which is, I believe, actually the case at present.

methodologies" will in fact turn out to be DMs, and others will be this new conceptual constellation I have proposed, MDMs.

Conclusions

In the course of using a Kuhnian analysis for the emergence of a family therapy group, I have found it necessary to make several modifications or additions to Kuhn's revised schema. Using Masterman's critique, I have regarded psychology as a multiple paradigm and multiple-DM discipline, and have chosen to use a narrow rather than broad interpretation of the paradigm and DM concepts. Thus, both are used only with respect to characteristics and criteria delineated by Kuhn, or Masterman. Additionally, I have emphasized the relationship of DM and methodology, drawn attention to the problem in identifying the "inclusive perimeter" of a DM, and proposed a "border of applicability" which delimits the practical and conceptual bounds of a paradigm and points to an important difference between disconfirmation and anomaly. While using the narrow interpretation of the paradigm and DM concepts, I found that this usage was quite compatible with events in the development of the DB family therapy group, though not sufficient to explain the behavioral-internal processes controversy, the 1959 split, or DM clustering within family therapy. For these reasons, I have proposed the meta-DM concept. With this concept included in the Kuhnian analysis, the emergence of the DB DM was fully interpreted and an analysis of the present felt crisis will very probably be productively interpretable and helpful in

resolving certain aspects of the crisis if the distinction between, and relationships among, methodology, DM and MDM are kept clear. This type of Kuhnian analysis will require detailed knowledge of the particular controversy, problem area, or issue; as such, these Kuhnian analyses will probably be most helpful if carried out by individuals or groups close to, but not directly involved in the particular issue. If such an analysis were attempted by someone not intimately familiar with the issues and the history or development of the problem, the results could miscarry. What is required is a detailed knowledge of this revised Kuhnian analysis, keeping the levels of conceptualization quite clear, and a specific and detailed knowledge of the relevant problem area. Under these conditions, I would see such an analysis as highly beneficial to the relevant area, and as at least one way to begin dissolving the impasse and developing a psychology of many MDMs, all of which would be appropriate to the meaning of the phenomenon they were attempting to study.

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